ŪK-H06305-16-p16298

CALCUTTA UNIVERSITY
Bco: No.............
Book No.............
LIBRARY.

105
·003

THE
BRITISH JOURNAL
OF
PSYCHOLOGY

16

CAMBRIDGE UNIVERSITY PRESS
LONDON: FETTER LANE, E.C. 4

H. K. LEWIS & CO., LTD., 136, GOWER STREET, LONDON, W.C. 1
WHELDON & WESLEY, LTD., 2–4, ARTHUR STREET, NEW OXFORD
STREET, LONDON, W.C. 2
CHICAGO: THE UNIVERSITY OF CHICAGO PRESS
(Agent for the United States)
BOMBAY, CALCUTTA, MADRAS: MACMILLAN & CO., LTD.
TOKYO: THE MARUZEN-KABUSHIKI-KAISHA

All rights reserved

THE

BRITISH JOURNAL OF PSYCHOLOGY

GENERAL SECTION

EDITED BY

F. C. BARTLETT

WITH THE ASSISTANCE OF

C. BURT, J. DREVER, AND S. S. ISAACS

AIDED IN THE SELECTION OF PAPERS BY

F. AVELING
E. A. BOTT
W. BROWN
B. EDGELL
G. DAWES HICKS
W. McDOUGALL
C. S. MYERS
T. H. PEAR
CARVETH READ
A. F. SHAND
C. S. SHERRINGTON
C. SPEARMAN
H. J. WATT
G. UDNY YULE

VOLUME XVI 1925—26

CAMBRIDGE
AT THE UNIVERSITY PRESS
1926

P16298

PRINTED IN GREAT BRITAIN

CONTENTS OF VOL. XVI

PART 1. JULY, 1925

PAGE

Obituary Notice: Professor James Ward (with Portrait). By W. E. JOHNSON 1

The Causes of the Continuous Change of Resistance observed in Psycho-galvanic Experiments. By ROBERT H. THOULESS 5

Feeling, Imaging and Thinking. By F. C. BARTLETT . . 16

A Comparative Study of the Mathematical Ability of Boys and Girls in Secondary Schools. By ANNIE E. CAMERON . 29

A Note on the 'Psychogalvanic' Reactions of Anaesthetised Cats. By F. AVELING and R. J. S. MCDOWALL . . 50

On the Intuitive Capacity of Children to understand Spoken Language. By J. W. TOMB 53

Critical Notice: Koffka's *Growth of Mental Life*. By CHARLES FOX 56

Publications Recently Received 62

Notes on Recent Periodicals 69

Proceedings of the British Psychological Society . . . 76

Eighth International Congress of Psychology . . . 76

British Psychological Society, Names of Officers and Members 1925 and Rules . . . . *at end*

PART 2. OCTOBER, 1925

Motives in acquiring Skill. By ISABEL BURNETT and T. H. PEAR 77

Note on the Sampling Error of Tetrad Differences. By C. SPEARMAN and K. HOLZINGER 86

Character and Temperament Tests. A Preliminary Report. By MARY COLLINS 89

Some Factors of Effectiveness in Mental ("Intelligence") Tests. By S. A. HAMID 100

The Coexistence and Localization of Feeling. A Rejoinder. By A. WOHLGEMUTH 116

The Social Distribution of Intelligence in the Isle of Wight. By HECTOR MACDONALD 123

A Note on the Psycho-Galvanic Reflex considered in conjunction with Estimates of Character Qualities. By W. S. BROWN . 130

Further Comment on the Psychological Significance of the Galvanic Reaction. By DAVID WECHSLER . . . 136

Publications Recently Received 140

Notes on Recent Periodicals 148

Proceedings of the British Psychological Society . . . 157

British Psychological Society 158

PART 3. JANUARY, 1926

PAGE
The Standpoint of Psychology. By F. AVELING 159
Converging Lines in Contemporary Psychology. By MARY WHITON CALKINS 171
Some Experiments upon Inhibition and Suggestion. By L. S. PENROSE 180
On Thermal Sensitivity and the Nature of Sensory Adaptation. By ELMER CULLER 193
Factors in Mental and Scholastic Ability. By H. G. STEAD . 199
On Certain Effects of Long Spells of Repetitive Work. By M. D. VERNON 222
The Discrimination of Wool Fabrics by the Sense of Touch. By HENRY BINNS 237
Obituary: Dr Henry J. Watt 248
Critical Notice: Burt's *The Young Delinquent*. By E. O. LEWIS 249
Publications Recently Received 254
Notes on Recent Periodicals 257
Proceedings of the British Psychological Society 263

PART 4. APRIL, 1926

Three Experiments on the Localization of Tones. By H. BANISTER 265
The Effects on Vision of Replacing Continuous by Flickering Illumination. By H. HARTRIDGE. R. J. LYTHGOE and W. V. MATTHEWS 293
Chain Instincts in Lambing Sheep. By A. H. H. FRASER . . 310
Group Tests in Reasoning Ability. By A. BARBARA DALE . 314
The Psychology of Conation and Volition. By F. AVELING . 339
Personality and Vocation. A Note on Effects of Introversion on Dominant Interests. By W. V. BINGHAM 354
Obituary: Professor Joseph Brough 363
Committee for Research in Education 365
Publications Recently Received 368
Notes on Recent Periodicals 371
Proceedings of the British Psychological Society 373

LIST OF AUTHORS

PAGE

Aveling, F. The Standpoint of Psychology 159

Aveling, F. The Psychology of Conation and Volition 339

Aveling, F. and McDowall, R. J. S. A Note on the 'Psycho-galvanic' Reactions of Anaesthetised Cats 50

Banister, H. Three Experiments on the Localization of Tones 265

Bartlett, F. C. Feeling, Imaging and Thinking 16

Bingham, W. V. Personality and Vocation. A Note on Effects of Introversion on Dominant Interests 354

Binns, Henry. The Discrimination of Wool Fabrics by the Sense of Touch 237

British Psychological Society 158

British Psychological Society, Names of Officers and Members 1925 and Rules *at end*

Brown, W. S. A Note on the Psycho-Galvanic Reflex considered in conjunction with Estimates of Character Qualities 130

Burnett, Isabel and Pear, T. H. Motives in acquiring Skill 77

Calkins, Mary Whiton. Converging Lines in Contemporary Psychology 171

Cameron, Annie E. A Comparative Study of the Mathematical Ability of Boys and Girls in Secondary Schools 29

Collins, Mary. Character and Temperament Tests. A Preliminary Report 89

Committee for Research in Education 365

Culler, Elmer. On Thermal Sensitivity and the Nature of Sensory Adaptation 193

Dale, A. Barbara. Group Tests in Reasoning Ability 314

Eighth International Congress of Psychology 76

Fox, Charles. Critical Notice: Koffka's *Growth of Mental Life* 56

Fraser, A. H. H. Chain Instincts in Lambing Sheep 310

Hamid, S. A. Some Factors of Effectiveness in Mental ("Intelligence") Tests 100

Hartridge, H., Lythgoe, R. J. and Matthews, W. V. The Effects on Vision of Replacing Continuous by Flickering Illumination 293

PAGE
JOHNSON, W. E. Obituary Notice of Professor James Ward . 1
LEWIS, E. O. Critical Notice: Burt's *The Young Delinquent* . 249
MACDONALD, HECTOR. The Social Distribution of Intelligence in the Isle of Wight 123
Notes on Recent Periodicals 69, 148, 257, 371
Obituary:
  Professor James Ward 1
  Dr H. J. Watt 248
  Professor J. Brough 363
PENROSE, L. S. Some Experiments upon Inhibition and Suggestion 180
Proceedings of the British Psychological Society 76, 157, 263, 373
Publications Recently Received . . . 62, 140, 254, 368
SPEARMAN, C. and HOLZINGER, K. Note on the Sampling Error of Tetrad Differences 86
STEAD, H. G. Factors in Mental and Scholastic Ability . 199
THOULESS, ROBERT H. The Causes of the Continuous Change of Resistance observed in Psycho-galvanic Experiments . 5
TOMB, J. W. On the Intuitive Capacity of Children to understand Spoken Language 53
VERNON, M. D. On Certain Effects of Long Spells of Repetitive Work 222
WECHSLER, DAVID. Further Comment on the Psychological Significance of the Galvanic Reaction 136
WOHLGEMUTH, A. The Coexistence and Localization of Feeling. A Rejoinder 116

BRITISH PSYCHOLOGICAL SOCIETY

NAMES OF OFFICERS AND MEMBERS, 1925[1]

OFFICERS

PRESIDENT: PROF. C. SPEARMAN, F.R.S.

VICE-PRESIDENT: C. S. MYERS, M.A., M.D., SC.D., F.R.S.

HON. GENERAL SECRETARY: REV. F. AVELING, PH.D., D.SC., D.D.

HON. TREASURER: W. H. B. STODDART, M.D., F.R.C.P.

LIBRARIAN: J. C. FLÜGEL, B.A.

EDITORS: (General Section) F. C. BARTLETT, M.A.; (Medical Section) T. W. MITCHELL, M.D.

SECTIONAL HON. SECRETARIES: (Education Section) W. H. A. DOCKERILL; (Medical Section) JOHN RICKMAN, M.A., M.B., B.CH., R. M. RIGGALL, M.R.C.S., L.R.C.P.; (Industrial Section) GEO. H. MILES, D.SC.; (Aesthetics Section) MISS N. G. R. TAYLOR.

MEMBERS OF COUNCIL

REV. F. AVELING, PH.D., D.SC., D.D.
P. B. BALLARD, M.A., D.LITT.
F. C. BARTLETT, M.A.
C. H. BOND, C.B.E., M.D., D.SC., F.R.C.P.
E. BULLOUGH, M.A.
D. J. COLLAR, M.A.
MISS B. EDGELL, M.A., PH.D.
ERIC FARMER, M.A.
J. C. FLÜGEL, B.A.
BERNARD HART, M.D.
MRS ISAACS, M.A.
C. W. KIMMINS, M.A., D.SC.
T. W. MITCHELL, M.D.
C. S. MYERS, M.A., M.D., SC.D., F.R.S.
T. H. PEAR, B.SC.
S. J. F. PHILPOTT, B.SC.
STANFORD READ, M.D.
JOHN RICKMAN, M.A., M.B., B.CH.
R. M. RIGGALL, M.R.C.S., L.R.C.P.
W. H. B. STODDART, M.D., F.R.C.P.
MISS N. G. R. TAYLOR.

---

[1] It is requested that notification of any errors in this list be sent to Miss M. L. St George, 121 A, Gloucester Road, South Kensington, S.W. 7

MEMBERS.

M. = Medical Section. E. = Education Section. I. = Industrial Section.
Ae. = Aesthetics Section.

M. Abelson, A. R., D.Sc., M.R.C.S., L.R.C.P., 40, Anson Road, Cricklewood, N.W. 2.
Ae. Abercrombie, Prof. Lascelles, The University, Leeds.
Ae. Ablett, L. R., F.R.G S., F.G.S., 50, Queen Anne's Gate, S.W. 1.
Adam, J. F., M.A., Training College, Jordanhill, Glasgow.
E. Adams, Prof. John, M.A., B.Sc., c/o Miss Grassam, 16, Bond Street, Holford Square, W.C. 1.
Adams, Stephen, B.Sc., 7, Florence Street, Moss Side, Manchester.
E. Adamson, J. E., Education Dept., Pretoria, S. Africa.
Adrian, E. D., M.D., M.R.C.P., Trinity College, Cambridge.
Aggrey, Dr J. E. K., Phelps-Stokes Fund, 297, Fourth Avenue, New York City, U.S.A.
Aitken, G. J., M.A., Ed.B., 35, Rowallan Gardens, Broomhill, Glasgow.
Akroyd, Fred., B.A., The Nook, 2, Belmont Road, Bushey, Herts.
M.E.I. Alcock, J. A. M., A.R.N.
M. Alderson, Mrs J. G., Coquetdale House, Marine Terrace, Blythe, Northumberland.
Alexander, Prof. S., M.A., F.B.A., 24, Brunswick Road, Withington, Manchester.
E. Alford, J., M.A.
Allen, Miss E. A., B.A., 14, Harrington Square, N.W. 1.
E. Allen, Miss E. P., The Grammar School, Carmarthen, S. Wales.
E. Allmond, Miss E. C., B.Sc., 90, Revelon Road, Brockley, S.E. 4.
M.E.I. Anderson, W. L. G., M.B., Ch.B., 93, Harley Street, W. 1.
E. Andrews, V. W., Felphen, Mulgrave Road, Sutton, Surrey.
M.E.I. Archdale, M. A., M.B., Sunderland Mental Hospital, Ryhope, Sunderland.
Armstrong, W. E., M.A., The Archaeological and Ethnological Museum, Cambridge.
E. Arundale, G. S., M.A., LL.B., Sevashrama, Adyar, Madras, S. India.
Ashton, Dr. E., The Limes, West Smethwick, Birmingham.
M.E.I. Austin, Eric, Toynbee Hall, 28, Commercial Street, E. 1.
M. Aveling, Rev. F., Ph.D., D.D., D.Sc., 8, Thayer Street, W. 1.
E. Axton, Edward H., B.A., "Navenby," Gerard Road, Rotherham.
E. Bailey, Miss E., 7, Walerand Road, Lewisham, S.E. 13.
E. Ball, Mrs H.
E. Ballard, P. B., M.A., D.Litt., 148, Sutton Court Road, Chiswick, W. 4.
M. Banham-Bridges, Miss K., Medical Bldg., McGill University, Montreal.
Banister, H., The Manor House, Grantchester, Cambridge.
Barbor-Might, L.
M. Baring, Lady Ulrica, 26, Hyde Park Street, W. 2.
M. Barkas, Mary R., M.B., B.S., 46, Connaught Street, W. 2.
E. Barlex, H. N., M.A., Inspector of Schools, Marribri, N.S.W., Australia.
Barr, Miss A. M., Grey-garth, Bilton Lane, Harrogate.
Barrow, Mrs., Lawn House, Carpenter Road, Edgbaston, Birmingham.
Bartlett, Mrs E. M., Turfcote, Storey's Way, Cambridge.
E.I.Ae. Bartlett, F. C., M.A., Psychological Laboratory, Cambridge.
M.E.I. Bartlett, R. J., M.Sc., A.R.C.S., 3, Eastcote Road, Harrow.
M. Barton, P. F., M.A., M.B., 98, Portland Place, W. 1.
M. Baynes, H. G., M.B., M.R.C.S., L.R.C.P., 24, Campden Hill Square, W. 8.
E. Bazeley, Miss Elsie T., B.A., Warrington Training College, Warrington, Lancs.
E. Beard, Miss M. J., Putney High School, Putney Hill, S.W. 15.
I. Beaver, C., 1, Bell Place, Stamford Road, Bowdon.
M. Bedingfield, Harry, D.S.O., M.B., B.Ch., 55, Headingley Lane, Leeds.
M.E.I. Bell, Andrew, "Carradale," 57, Erith Road, Belvedere, Kent

Ae. BERESFORD, J. D.
M. BERKLEY HILL, Major O. A. R., I.M.S., M.A., M.D., Medical Superintendent, Ranchi European Mental Hospital, Ranchi (Bihar and Orissa), India.
BERLYN, J. A., M.B., 65, Wheeley's Road, Birmingham.
E. BERNSTEIN, E., 49, Cable Street, E. 1.
I. BEVINGTON, Miss S. M., B.Sc., 12, Chenies Street Chambers, W.C. 1.
M. BHAJEKAR, M. V., 22, Farndon Road, Oxford.
BICKERSTETH, Miss, 9, Melville Street, Edinburgh.
M.E.I.Ae. BINNS, H., 11, Toller Drive, Heaton, Bradford.
BIRCH, Rev. R. S., M.A., 58, East Restalrig Terrace, Leith.
M.E. BIRCHENOUGH, C., M.A., 103, Tonbridge Road, Maidstone.
BJARNASON, Prof. AUGUST, Ph.D., Reykjavik, Iceland.
M. BLACKER, Capt. C. P., Balliol College, Oxford.
BLACKLEDGE, Miss L., Education Offices, Margaret Street, Birmingham.
M. BLAIR, C. J. LONGWORTH, M.R.C.S., L.R.C.P., 1, Cardigan Gate, Richmond, Surrey.
M. BLIGH, S., 28, Grosvenor Road, Westminster, S.W. 1.
M.E.Ae. BODÉ, Miss O., 2, Wentworth Mansions, Keat's Grove, Hampstead, N.W. 3.
M.E.I.Ae. BODKIN, Miss A. M., M.A., 50, Southmore Lane, Highgate, N. 6.
BOLAM, W. H., 51, York Road, Hall Green, Birmingham.
M.E. BOLTON, S. J., M.D., D.Sc., F.R.C.P., West Riding Asylum, Wakefield, Yorks.
M. BOND, C. H., C.B.E., M.D., D.Sc., F.R.C.P., Board of Control, 66, Victoria Street, S.W. 1.
BOOLE-STOTT, Miss MARY, B.A., 26, Sussex Place, Regent's Park, N.W. 1.
BOOTH, F., Education Offices, Margaret Street, Birmingham.
I. BOSTOCK, H. J., c/o Bostock & Co., Boot and Shoe Mfrs, Stafford.
M.E. BOUSFIELD, E. G. P., L.R.C.P., 7, Harley Street, W. 1.
M.E.I. BOUSFIELD, STANLEY, M.D., 10, Albion Street, Hyde Park, W. 2.
E. BOWERLEY, Miss MARY, M.A., 14, Eton Villas, S. Hampstead, N.W. 3.
E. BOWMAN, Miss F. L., 36, Richmond Road, Oxford.
M. BOYD, Dr STANLEY, M.R.C.S., L.R.C.P., 4, Leeds Road, Harrogate.
BOYD-BARRETT, Rev. E., M.A., Ph.D., Mungret College, Limerick, Ireland.
M.E.I. BOYLE, Miss HELEN, M.D., L.R.C.P., 9, The Drive, Hove, Sussex.
BRACEY, H. C., 6, Elmden Road, Acock's Green, Birmingham.
E. BRACKENBURY, Miss L., M.A., L.C.C., Graystoke Place Day Training College, Bream's Buildings, E.C. 4.
E. BRADDOCK, A. P., M.A., B.Sc., 82, Kingsbury Road, Erdington, Birmingham.
E. BRADFORD, E. J. G., B.Sc., The University, Sheffield.
M. BRAMWELL, EDWIN, M.D., F.R.C.P., 23, Drumsheugh Gardens, Edinburgh.
BRANSON, Dr GUY J., 272 Hagley Road, Edgbaston, Birmingham.
M. BREND, W. A., M.A., M.D., B.Sc., 14, Bolingbroke Grove, S.W. 11.
E. BRERETON, A., B.A., Norton House, Saffron Walden, Essex.
Ae. BRERETON, CLOUDESLEY, 9, Highbury Terrace, Highbury, N. 5.
M. BRIERLEY, Mrs M., B.Sc., Granary Cottage, Crabtree Lane, Harpenden, Herts.
BROCK, A. CLUTTON, The Red House, Godalming, Surrey.
BROOKE, Miss PHOEBE M., M.A., Silvana, Clarkston, Glasgow.
Ae. BROUGH, Prof. J., M.A., 12, Carlingford Road, Hampstead, N.W. 3.
E. BROWN, Capt. J., M.A., 93, Turney Road, Dulwich Village, S.E. 21.
M.E.I. BROWN, WILLIAM, M.A., M.D., D.Sc., M.R.C.P., 88, Harley Street, W. 1.
M. BROWN, WARBURTON, 11, Weymouth Street, Portland Place. W. 1.
E BROWN, W. S., 280, Archway Road, N. 6.
M. BROWNE, LEONARD F., M.D., 9, Harley Street, W. 1.
M.E.I. BRYAN, DOUGLAS, M.R.C.S., L.R.C.P., 72, Wimpole Street, W. 1.
M.E. BRYANT, Miss E. M., M.A., 24, Brodrick Road, Wandsworth Common, S.W. 17.
M. BRYCE, W. H., M.B., C.M., Kenlaw House, Colinsburgh, Fife.
M. BRYCESON, D. EBENEZER, 59, Queen Anne Street, W. 1.
E.Ae. BUDGE, Miss, Bolton Girls' School, Park Road, Bolton, Lancs.
Ae. BULLEY, Miss M., 19, Taviton Street, Gordon Square, W.C. 1.
M.E.Ae. BULLOUGH, E., M.A., 6, Huntingdon Road, Cambridge.

	Burke, J. B., M.A., 63, St James' Street, S.W. 1.
M.	Burke, Noel H. M., 65, Cheyne Walk, Chelsea, S.W. 3.
M.E.I.	Burt, C., M.A., 30, Princess Road, Regent's Park, N.W. 1.
M.E.	Buysman, Miss H. C., Elsinore, 12, Daysbrook Road, S.W. 2.
	Caldicott, Charles H., M.B.E., M.B., Grantbourne, Chobham, Woking.
	Caley, Miss Clarice, B.A., University Women's Club, 2, Audley Sq., W.
E.	Cameron, Miss Annie E., M.A., 24, Langbourne Avenue, Highgate, N. 6.
M.E.I.	Cameron, Miss H. M.
E.	Campbell, Miss I. E., M.A., B.Ed., Firhill, Colinton, Edinburgh.
E.	Campbell, Col. R. B., Headquarters, Physical Training School, Aldershot.
M.E.I.	Carey, Miss N., D.Sc., 28, Quadrant Road, Canonbury, N. 1.
	Carr, Prof. H. Wildon, D.Litt., 107, Church Street, Chelsea, S.W. 3.
	Carr, Capt. W. B. H., 6, Sackville Road, Hove, Sussex.
E.	Carter, Miss M. V.
M.	Carver, A. E. A., M.A., M.D., 59, Wellington Road, Edgbaston, Birmingham
	Caughey, J. W., 29, Benson Road, Forest Hill, S.E. 23.
	Cavenagh, Prof. F. A., M.A., Singleton Park, Swansea.
M.E.	Chadwick, Miss M. W., 6, Guilford Place, W.C. 1.
	Chambers, D., 7, Charlotte Road, Edgbaston, Birmingham.
E.	Champion, H. B., 69, Oakfield Road, Old Southgate, N. 14.
E.	Chapman, Albert E., B.A., Department of Education, The University, Birmingham.
M.	Chatterton, Dr Harold, M.R.C.S., L.R.C.P., 22, Weymouth Street, W. 1.
	Chevassut, Rev. F. G., M.A., B.Sc., Toc H, Victoria Park, Manchester.
E.I.	Chivers, John, J.P., "Wychfield," Huntingdon Road, Cambridge.
E.	Churchill, Lord Ivor Spencer, 12, North Audley Street, W.C.
	Clare, Rev. Lawrence, 153, Portland Road, Edgbaston, Birmingham.
E.	Claremont, C. A., B.Sc., "Hawarden," 7, West Heath Avenue, Hampstead.
M.	Clarke, Miss Dorothy J., Nutford House, Nutford Place, Marble Arch, W. 1.
E.	Clarke, Miss W. S., 17, Brighton Grove, Rusholme, Manchester.
E.	Clarkson, Miss M., St Mary's College, Cheltenham.
	Cloake, Philip C. P., 27, Owlestone Road, Cambridge.
	Cobb, G. F., M.R.C.S., County Medical Hospital, Burntwood, Lichfield.
E.	Coetzee, J. Chr., M.A., Reitz Street, Potchefstroom, S. Africa.
M.	Coghlan, J. C.
	Coit, S., Ph.D., 30, Hyde Park Gate, S.W. 7.
M.E.	Cole, R. H., M.D., F.R.C.P., 25, Upper Berkeley Street, W.
	Coles, P. B., M.A., 148, West Hill, Putney, S.W. 15.
E.	Collar, D. J., M.A., 8, Thirlmere Road, Muswell Hill, N. 10.
M.	Collins, M. A., M.D., Chartham Downs, nr Canterbury.
M.E.	Collins, Miss M., M.A., Board of Education, S.W. 1.
M.E.I.	Collins, Miss Mary, M.A., B.Sc., Psychological Dept., The University, Edinburgh.
I.	Collis, Prof. E. L., M.B., The Lodge, Llandaff, nr Cardiff.
E.	Columbine, Miss E. M., Pietermaritzburg, Natal, South Africa.
M.	Connell, E. H., M.B., Ch.B., 7, Greenhill Gardens, Edinburgh.
	Conolly, M., 15, Highfield Road, Edgbaston, Birmingham.
M.E.	Cooper, J. W. A., L.R.C.P. & S., 1, Clarence Terrace, Newcastle-on-Tyne.
M.	Corr, D. E., M.D., 26, St John Street, Manchester.
M.E.I.	Cotton, Charles Kenneth, B.A., Ranfurly Castle School, Bridge of Weir, Renfrewshire.
M.	Court, A. C., M.D.
	Coupland, W. H., L.R.C.P., Albert House, Haverbreakes, Lancaster.
	Cowan, D. M. W., 66 G, Talford Road, S.E. 15.
E.	Cox, J. W., M.Sc., 184, Northumberland Park, Tottenham, N. 17.
E.	Cracknell, Sidney H., M.Sc., 49, Manwood Road, Crofton Park, S.E. 4.
M.E.	Crosthwaite, Mrs A. A., Highfield, King's Langley, Herts.
M.Ae.	Cubberley, A. J., 14, Kersland Street, Hillhead, Glasgow.
E.	Cull, E., B. Sc., Education Dept., University, Edmund St., Birmingham.

M. Culpin, Millais, M.B., F.R.C.S., The Meads, Loughton, Essex.
E. Currey, Miss S. E., L.R.A.M., A.R.C.M., 104, Canterbury Grove, West Norwood, S.E. 27.
E. Curtis, S. J., B.A., Education Department, University, Leeds.
E.I. Dainow, M., B.Sc., 17, Gower Street, W.C. 1.
E. Dale, Miss A. B., M.A., Ph.D., 22, Berkeley Square, W.
E. Dall, A. W., M.A., Maisonette, Maybury Road, Woking.
M.I. Darwin, Sir H., K.B.E., F.R.S., The Orchard, Huntingdon Road, Cambridge.
M.E.I. Darwin, Miss Ruth, The Orchard, Huntingdon Road, Cambridge.
E. Davey, Miss C. M., 29, Westall Street, Hyde Park, Adelaide, S. Australia.
Davies, The Hon. Rev. Albert E., M.A., 14, South Parade, Whitley Bay, nr Newcastle-on-Tyne.
E.Ae. Davies, Miss Ethel, 7, Brunswick Square, W.C. 1.
E. Davies, E. T., M.A., Education Offices, Haverfordwest.
E. Davson, Alan W., F.S.I., 42, Lansdowne Crescent, Holland Park, W. 11
M. Davy, W. B., M.R.C.S., 348, Finchley Road, Hampstead, N.W.
E. Dawes, Rev. H. V., B.A., Severn Braw, Bridgnorth, Salop.
M. Dawson, G. de H., M.R.C.S., L.R.C.P., 138, Lauderdale Mansions, Maida Vale, W. 9.
M.E.I. Dawson, S., M.A., D.Sc., Hazel Bank, Bearsden, Dumbartonshire.
M. Dawson, W. S., M.A., M.D., M.R.C.P., The Maudesley Hospital, Denmark Hill, S.E. 5.
Ae. Dent, E. J., M.A., 10, New Quebec Street, W. 1.
M.Ae. Dent, Miss E. Margaret, 18, Ampthill Street, Norwich.
M.E. Devine, H., M.D., O.B.E., F.R.C.P., Corporation Mental Hospital, Portsmouth.
M. Devine, Dr James, Donard Lodge, Walton, Liverpool.
M. Dillon, F., M.D., Ch.B., Northumberland House, Green Lanes, Finsbury Park, N. 4.
Dixon, Major E. T., Billy Dun, Half Way Tree, Jamaica.
E. Dobson, R. R., The Grammar School, Cheltenham.
E. Dockerill, W. H. A., M.A., 75, Ringford Road, East Putney, S.W. 18.
I. Dowdall, His Hon. Judge H. O., K.C., Melfort Cottage, Boar's Hill, Oxford.
M. Downey, Rev. R., D.D., Mission House, Brondesbury Park, N.W. 6.
M.E.I. Drever, James, M.A., B.Sc., D.Phil., Psychology Department, University, Edinburgh.
M. Drummond, Miss M., M.A., 10, Hartington Gardens, Edinburgh.
M.E. Duff, Miss I. F. Grant, 22, Mecklenburg Square, W.C. 1.
M. Eager, R., M.D., O.B.E., The Devon Mental Hospital, Exminster.
M. Eder, M. D., M.D., 2, Harley Place, Harley Street, N.W. 1.
E.I.Ae. Edgell, Miss B., M.A., D.Sc., 15, Lyon Road, Harrow.
M. Ehrenberg, M. I., 143, Grosvenor Road, N. 5.
M. Elliot, G., "Whirlow," Harley Street, Leigh-on-Sea, Essex.
M.E. Elrington, Prof. G. A., D.Sc., F.I.S., Blackfriars, Oxford.
Enginas, A. J., 12–15 Upper Bedford Place, Russell Sq., W.C. 1.
E. Ensor, Mrs Beatrice, 11, Tavistock Square, W.C. 1.
Eustace, Miss Violet, 25, Bedford Gardens, Kensington, W. 8.
E. Evans, Capt. John, B.A., 9, Victoria Park Road, Cardiff, Wales.
Fairweather, A. C., Hollymore Annexe, Nothfield, Birmingham.
I. Farmer, Eric, M.A., 28, Comeragh Road, W. 14.
E. Fayerman, Miss W. M., Kilmarnock, London Road, Tonbridge.
Fazan, E. A. C., M.C., M.R.C.S., Belmont, Wadhurst, Sussex.
Feasey, Miss L., M.A., 12, Oakbrook Road, Sheffield.
E. Fenner, Miss H. R., 221, Barry Road, S.E. 22.
E. Fennings, A. J., 9, Holmbury View, Springfield, Upper Clapton, E. 5
Ferguson, Mrs. B., 521, City Road, Birmingham.
M. Ferguson, F., Queen's College, Oxford.
E.I. Ferguson, R. W., B.Sc., A.R.C.S., 36, Linden Road, Bournville, Birmingham
E.I. Field, G. C., M.A., B.Sc., The University, Liverpool.

M.E. Fildes, Miss A. F., Hockerill College, Bishops Stortford.
M.E. Fildes, Miss L. G., B.A., 35, Brunswick Gardens, Maid's Causeway, Cambridge,
M.E. Fitch, Arthur A., M.R.C.S., L.R.C.P., 12, Falkner Square, Liverpool.
M. Fitzgerald, Gerald H., 11, Weymouth Street, W. 1.
M. Fleming, Gerald W. T. H., M.R.C.S., L.R.C.P., Mental Hospital, Ryhope, Sunderland.
M. Flower, Rev. J. Cyril, M.A., 84, Chesterton Road, Cambridge.
M.E.I.Ae. Flügel, J. C., B.A., 11, Albert Road, N.W. 1.
M.E.I. Flügel, Mrs J. C., 11, Albert Road, N.W. 1.
M.E.I.Ae. Flügel, Mrs Mary, 54, Harley House, N.W. 1.
M. Flynn, Rev. Thos. E., M.A., St Joseph's College, Upholland, Wigan, Lancs.
M.E.I. Forbes, Chas., M.B., O.B.E., 54, King Street, Aberdeen.
Forster, L. S., M.A., B.Sc., The University, Hong Kong.
M. Forsyth, C. W., M.D., Rubery Hill Mental Hospital, nr Birmingham.
M.E. Forsyth, D., M.D., 9, Harley Street, W. 1.
M. Fowler, H. L., M.A., Albany High School, Western Australia.
E. Fox, C., M.A., Warkworth House, Cambridge.
M.E. Fox, Miss Evelyn, Central Association for Mental Welfare, 24, Buckingham Palace Road, S.W. 1.
M.E. Fox, Miss M. E., 7, The Broadway, Woodford Green.
E. Fox, Miss M. E., County School for Girls, Beckenham, Kent.
M.E. Fox, Miss M. G., 30, Talbot Road, Bayswater, W. 2.
M. Franklyn, Miss M. E., M.B., B.S., 50, Porchester Terrace, W. 2.
M.E. Franklyn, Miss Olive N., 44, Buckingham Palace Mansions, S.W. 1.
E. Franks, Miss H., 13, Brunswick Square, W.C. 1.
Fraser, J. A., M.A., B.Ed. c/o Industrial Fatigue Research Board, 272, Oxford Road, Manchester.
M.E. Frodsham, Miss M., B.Sc., 22, Somerset Terrace, W.C. 1.
Frost, I., 23, Mortimer Street, W. 1.
E. Fuller, Miss M., 2, Oxford Road, Sidcup, Kent.
M.E. Gabler, Mrs Rosalie, 129, King Henry's Road, N.W. 3.
Gallichan, Mrs N. K., Oakdene, Meadway, Gidea Park, Essex.
M. Gallichan, Walter M., Oakdene, Meadway, Gidea Park, Romford.
Gallie, Miss J. B., M.A., Training College, Jordanhill, Glasgow.
Ganera, Miss R., Ph.D., 43, General Parensor, 43, Sofia, Bulgaria.
M.E. Gans, Mrs Howard, 260, West 76th Street, New York City, U.S.A.
E. Garnett, C. M. J., O.B.E., M.A., 21, Well Walk, N.W. 3.
E. Gaw, Miss Frances, 14, Doughty Street, W. 1.
M. Gibson, H. C., M.B., Elm Bank, Dollar, Scotland.
M. Gillespie, C. M., M.A., The University, Leeds.
Ginsberg, M., M.A., London School of Economics, Houghton Street, Aldwych, W.C. 2.
M.E. Glaister, N., M.B., B.S. (Lond.), Weir Cottage, Chertsey, Surrey.
M.E. Glover, Edward, M.D., 6, Bentinck Street, W. 1.
M.E. Glover, J., M.B., Ch.B., 6, Bentinck Street, Cavendish Square, W. 1.
M.E. Good, T. S., M.R.C.S., L.R.C.P., Ashhurst Special Neurological Hospital, Littlemore, Oxon.
E. Gordon, Hugh.
M. Gordon, R. G., M.D., M.R.C.P., 6, Queen Square, Bath.
Goulston, William W., B.A., B.Sc., A.I.C., Shell-Mex, Ltd., Ramsden Dock, Barrow.
M.E.I. Goulston, Mrs B.
E. Grace, Miss A. L., 83, Purser's Cross Road, Fulham, S.W. 6.
Green, G. H., Dept. of Education, University College of Wales, Aberystwyth.
Green, H. M., B.A., LL.B., Fisher Library, University of Sydney, Australia.
M. Ae. Greig, John T. Y., M.A., Armstrong College, Newcastle-on-Tyne.
E.I. Griffith, Percy, M.I.C.E., F.G.S., F.R.S.I., 39, Victoria Street, S.W. 1.

Griffiths, Miss A. F., M.A., The Poplars, Norman's Place, Altrincham.
M. Hadfield, J. A., M.B., 9, Harley Street, W. 1.
E. Hamilton, E. R., University College of N. Wales, Bangor.
Hammond, Miss B. M., Regent's House, Regent Street, S.W. 1.
Hammond, Miss Margaret, B.A., Education Department, Birmingham University.
M. Hampton, F. A., M.B., Blue Spa, Chesham Bois, Bucks.
E.I. Handy, Edwin, 29, Kirkham Road, Horton, Bradford.
E. Hankey, Mrs Arthur, c/o Westminster Bank Ltd., 64, Western Road, Hove, Sussex
E. Hanlin, Rev. Frank, Student Movement House, 32, Russell Square, W.C. 1.
Harburn, Miss E. F., M.B.E., 16, Denning Road, Hampstead, N.W. 3.
M. Hardcastle, D. N., M.R.C.S., L.R.C.P., "Elmcroft," Aldenham Road, Bushey, Herts.
M. Hardy, Herbert Wm., M.R.C.S., L.R.C.P., 57, Carver Road, Herne Hill, S.E. 24.
M. Harford, C., M.B., 30, Finsbury Sq., E.C. 2.
M.E. Hargreaves, Rev. H. L., B.A., B.Mus., The Clergy House, 2, Spencer Hill, S.W. 19.
M.E.I.Ae. Hargreaves, Miss L. J., Southlands College, Battersea, S.W. 11.
M. Harper, R. S., M.R.C.S. (Eng.), L.R.C.P. (Lond.), F.R.M.S., Med. Supt. Min. of Pens. Neur. Clinic, B'ton., 36, First Av., Hove, Sussex.
Harris, U. A., M.A., 54, Leamington Ter., Edinburgh.
M. Hart, Bernard, M.D., 94, Harley Street, W. 1.
Harvey, N. W., M.A., The Homestead, Selly Oak, Birmingham.
Hawkins, Miss Frances E., M.A., 16, Pembridge Crescent, W. 11.
Hawtrey, Miss F., Avery Hill Training College, Eltham, S.E. 9.
E. Hayes, A. E., F.R.S.A., 13, Priory Gardens, Highgate, N. 6.
Hayes, E. D., M.B., B.Ch., D.P.M., Assistant M.O., Croydon Mental Hospital, Warlingham, Surrey.
M.E.Ae. Hazlitt, Miss V., M.A., Bedford College, Regent's Park, N.W.
M. Head, H., M.D., F.R.S., 52, Montague Square, W. 1.
Hearn, Miss B., Collegiate Hall, Sheffield.
E. Heath, A. E., M.A., The University, Liverpool.
I. Heathcote, H. L., B.Sc., M.Sc., F.I.C., Rudge-Whitworth, Ltd., Sparkhill, Birmingham.
M. Hemingway Rees, Mrs Mary I., M.R.C.S., L.R.C.P., 146, Harley Street, W. 1.
M.Ae. Henty, Miss Sybil, 103, Belgrave Road, S.W. 1.
E. Hepburn, Wm. A. F., M.A., B.Ed., 37, Dene Avenue, Lemington-on-Sea, Northumberland.
M.E. Herford, Mrs M. B., M.B., B.Sc., 19, Redlands Road, Reading.
I. Hitchens, W. L., 3, Central Buildings, Westminster, S.W. 1.
M.I. Hicks, Prof. G. D., M.A., Ph.D., 9, Cranmer Road, Cambridge.
E. Hilda, Sister Marie, Notre Dame, Dowanhill Training College, Glasgow.
Hingley, Rev. R. H., B.A., 1, Montgomery Street, Hollinwood, Oldham.
M. Hoernle, Prof. R. F. A., M.A., B.Sc., The University of the Witwatersrand, Johannesburg.
E. Hollander, Bernard, M.D., 57, Wimpole Street, W. 1.
M.E.I. Hooper, S. E., M.A., Glen Cottage, Cookham Dean, Berks.
M.E.I.Ae. Hope, Thomas, M.A., B.Ed., 17, Wilton Road, Edinburgh.
Hopkins, Prince, M.A., c/o London Midland Bank, Ltd., 38, Brompton Road, S.W. 3.
E. Howard, C., B.A., St Luke's College, Exeter, Devon.
M. Howe, Eric, 40, Bessborough Street, W. 1.
E. Howlett, Miss E., 2, Tabley Road, N. 7.
E.I. Hubert, A., 7, Hampstead Gardens, Golder's Green, N.W. 4.
M. Hudson, Dr E., M.R.C.S., L.R.C.P.
E. Hughes, Mrs A. G., B.A., 19, Cromer Terrace, Leeds.
E. Hughes, Miss E. M., County School, Tunbridge Wells, Kent.

E.	Hughes, Miss Maude, B.Sc., 44, Frognal, Hampstead, N.W. 3.
E.	Hume, Miss G., Orchard Corner, Westmount Road, Eltham, S.E. 9.
E.	Hunter, Mrs M. K., 14, St Simon's Avenue, Putney, S.W. 15.
M.	Hunter, P. D., M.R.C.S., L.R.C.P., Three Counties Asylum, Arlesey, Beds.
M.E.	Ingram, C. B., Moira House, Eastbourne.
M.	Inman, William S., M.B., 22, Clarendon Road, Southsea.
	Innes, C. E., 31, Randolph Crescent, W. 9.
	Inskip, The Hon. Janet, The Chantry, Abbots Leigh, nr Bristol.
M.	Isaacs, Mrs S., M.A., Malting House Gdn. School, Malting Lane, Cambridge.
E.	Jackson, Miss A. M., M.A., Torrington, 48, Pollards Hill East, Norbury, S.W. 16.
M.	Jago, W. J., M.R.C.S., L.R.C.P.
	James, H. E. O., B.A., The University, Manchester.
M.	Jeffreys, Harold, M.A., D.Sc., St. John's College, Cambridge.
	Jeffries, N., Greenhill Road, Moseley, Birmingham.
	Jenkins, Miss J. A., Edgehill Training College, Liverpool.
E.	Johnston, Miss Katherine L., M.A., Maria Grey Training College, Salisbury Road, N.W. 6.
E.	Jones, B. P., B.A., 15, Denmark Avenue, S.W. 19.
M.	Jones, D. W., M.R.C.S., L.R.C.P., 25, Hutt Street, Hull.
M.	Jones, E., M.D., M.R.C.P., 81, Harley Street, W. 1.
E.	Jones, Miss E., The Frogmore Mentally Defective School, Wandsworth, S.W. 11.
	Jones, Rev., H. A., All Saints' Vicarage, Small Heath, Birmingham.
	Jones, Mrs Lilian Aylmer, Lady Artists' Club, 5, Blythswood Square, Glasgow.
Ae.	Jones, Miss V. G. A., B.A., 3, Holly Lodge Gardens, West Hill, Highgate, N. 6.
E.	Joseph, Miss C. H., 81, Holland Park, W. 11.
	Joyce, Miss, A., Frederick Road, Edgbaston, Birmingham.
M.E.	Kay, J., B.Sc., 139, Victoria Road, Old Charlton, S.E. 7.
E.	Keatinge, M. W., M.A.
M.	Kelly, Simon, B.Sc., M.R.C.S., L.R.C.P., The Halderns, Singleton Road, Kersal, Manchester.
E.	Kennedy-Fraser, David, M.A., B.Sc., Jordanhill Training College, Chamberlain Road, Glasgow.
M.	Kenneth, J. H., M.A., F.R.G.S., The Homestead, Clynder.
I.	Kent, Prof. A. F. Stanley, Holm Ray, Iron Acton, Glos.
	Kenyon, Miss, 75, St George's Road, S.W. 1.
M.E.I.	Kimmins, C. W., M.A., D.Sc., National Liberal Club, Whitehall Place, S.W. 1.
E.	King, Miss E. M., B.A., The University, Sheffield.
E.	King, O. Bolton, Education Offices, Margaret Street, Birmingham.
M.E.	Kirby, Miss Mary, Nutford House, Nutford Place, W. 1.
E.	Kirkman, F. B., B.A., 5, Gerrard Street, W. 1.
E.	Kittermaster, Rev. D. B., M.A., Grove Hill House, Harrow.
I.	Klein, Major Adrian B., M.B.E., 32, The Pryors, East Heath Road, Hampstead Heath.
	Knowles, P., B.Sc., Southgate County School, Fox Lane, Palmers Green, N. 13.
M.	Korn, Morris, 101, Canfield Gardens, N.W. 6.
M.	Krause, R. A., M.D., D.Sc., Norwood, Kinghorn.
M.	Ladell, R. MacDonald, M.B., Ch.B., 395, Coventry Road, Small Heath, Birmingham.
M.	Laird, Prof. J., 4, Cranmore Gardens, Belfast.
I.	Lake, A. F., A.K.C., J.P., "Wrangaton," Bromley, Kent.
Ae.	Lall, Mrs D. M., The University (Education Dept.), Edmund Street, Birmingham.
	Lambie, C. G., M.C., M.B., Ch.B., 12, Merchiston Crescent, Edinburgh.
M.E.	Lane, L., L.R.C.P. & S. (Ed.), "Sunnydene," Wallace Avenue, Leagrave, Beds.

M. Langford, F. C. G., West Lodge, 142, The Rye, East Dulwich, S.E. 22.
E. Latta, Prof. R., M.A., D.Phil., 4, The College, Glasgow.
Laugher, J. H., 58, Horseley Heath, Tipton, Staffs.
Lavin, C. R., M.D., Sc.D., Ayala 38, Madrid, Spain.
E. Laws, A. R., B.Sc., 90, St George's Terrace, Newcastle-on-Tyne.
E. Lee, James, Farm School, Newton-le-Willows, Lancs.
I. Lee, K., Old House, Ashley Heath, Hale, Cheshire.
E. Le May, Miss C. M., 26, The Mall, Faversham, Kent.
E. Lewis, Mrs A. D., "Ashleigh," Cannon Hill Lane, Merton Park, S.W. 20.
M.E. Lewis, E. O., B.A., D.Sc., 29, Highbury New Park, Highbury, N. 5.
E.I.Ae. Lewis, H. R., B.A., 11, Upper Phillimore Gardens, W. 8.
M.E. Lewis, Miss, M. G., 16, Gordon Street, W.C. 1.
Liddle, Miss, M. E., Education Offices, Margaret Street, Birmingham.
E. Limpenny, Mrs E. M. F., Ruby Street (Infants) L.C.C. School, N. Camberwell, S.E. 15.
E. Lissa, Miss L. de, Gipsy Hill Training College, Norwood, S.E. 19.
I. Lloyd, H., Dol-Eglwys, Builth Wells, Breconshire.
M.E.I. Lloyd-Davies, Miss M. H., M.A., Fairfield, Brimstage, nr Birkenhead
E. Lloyd-Evans, Miss, M.A., Furzedown Training College, S.W. 17.
Lofthouse, Rev. Prof. W. F., Friary Road, Handsworth, Birmingham.
E. Loosli, Mrs M., 12, Richmond Gardens, Hendon, N.W. 4.
M.E. Lothian, A. J. D., 13, Dick Place, Edinburgh.
E. Loudon, Rev. Alexander, M.A., B.D., The Manse, Falkirk.
E. Louis, Miss V., 3, Stafford Mansions, Albert Bridge Road, S.W. 11.
E.I.Ae. Lovell, J. W. S., 52, Leslie Road, Finchley, N. 2.
M.E. Low, Miss B., B.A., 13, Guilford Street, W.C. 1.
M.E.I. Lowson, J. P., M.A., M.D., The University, Brisbane, Australia.
E. Ludolph, Miss L. M. C., 86, Harley Street, W. 1.
Lunniss, Miss B. A., B.A., 103, Finchley Road, N.W. 3.
M.E.Ae. Lyon, Miss J., 18, Mecklenburgh Square, W.C. 1.
E. (MacDonald) Sister Elizabeth, Notre Dame Secondary School, 17, Victoria Circus, Dowanhill, Glasgow.
Macgregor, Dr., 60, Harborne Road, Edgbaston, Birmingham.
Macintosh, Miss, Inverclay, 38, Coleshill Street, Sutton Coldfield, nr Birmingham.
E. Mackay, Miss M. E.
Mackay, R. T., B.A., c/o Bradford Dyers' Association Ltd. (Labour Dept.), 39, Well Street, Bradford.
M.E.I. MacKenzie, Sir W. L., M.A., M.D., 14, Belgrave Place, Edinburgh.
M. Macqueen, D., M.B., Ch.B., Bethnal Green Hospital, Cambridge Road, E. 2.
M. Manning, W. H. O'N., B.A., Trinity College, Cambridge,
M. Mapother, E., M.D., F.R.C.S., Maudsley Hospital, Denmark Hill, S.E. 5.
M. Maries, Edward de, 102a, Westbourne Grove, W. 2.
Mark, H., St John's College, Agra, U.P., India.
M.E.I. Marsden, R. E., B.Sc., 45, Milton Avenue, Wellsway, Bath.
M.E. Martin, O. S., M.R.C.S., L.R.C.P., 3, Duke Street, Manchester Square, W. 1.
M. Matthew, D., M.B., Ch.B., Rodgarth, 11, Rockfield Crescent, Dundee.
McAllister, Miss A. H., M.A., 49, Lawrence Street, Dowanhill, Glasgow.
McAllister, William, M.B., Ch.B., Craig House, Edinburgh.
M. McAlpine, W., L.R.C., 5, Clifton Place, Glasgow, W.
M.E.I. McClelland, William, M.A., B.Sc., B.Ed., Glebe Street, Dalkeith.
M. McCurdy, J. T., Corpus Christi College, Cambridge.
M.E.Ae. McFarlane, Miss M., Ph.D., 50, Southwood Lane, Highgate.
McGowan, H., 40, Monument Road, Birmingham.
M.E.I. McIntyre, J. L., M.A., D.Sc., Abbotsville, Cults, Aberbeen, N.B.
McKenzie, Rev., J., M.A., Principal, Wilson College, Bombay.
M. McKerrow, J. C., M.B., Ch.B., 34, Cartwright Gardens, W.C. 1.
McLeod, Miss Rebekah, c/o Doveton College House, Vepery, Madras, S. India.

E. McNabb, W., 65, Linden Avenue, Wembley, Middlesex.
E. McPetrie, J. M., Nadras College, St. Andrews.
Mears, W. G. A., M.A., High School, Germiston, South Africa.
Melson, G. H., M.D., 102, Alcester Road, Moseley, Birmingham.
M. Menon, M. P. Kesava, M.B., B.Ch., St Winifred's, 7, Church Street, N. 9.
Mercier, Miss Winifred, Whiteland's College, Chelsea, S.W. 3.
Messer, Andrew, M.B., B.Sc., Medical Officer of Health, Lemington-on-Tyne, Northumberland.
E. Metcalfe, A., 210, Camden Road, N.W. 1.
E. Michael, Miss M., B.A., LL.B., 39 a, Temple Fortune Mansions, Golder's Green, N.W.
Michotte, Prof. A., Laboratoire de Psychologie, 1, rue des Flamands, Louvain.
M. Middleton, A. G., M.D.
Ae. Middleton, Miss N. E., 8, Ashburnham Mansions, Chelsea, S.W. 10.
M. Milburn, Rev. R. G., Black Bridge, Beaulieu, Hants.
E.I. Miles, Dr. George H., D.Sc., c/o National Institute of Industrial Psychology, 329, High Holborn, W.C. 1.
Miller, A. G. J., B.Sc., 108, Woodwarde Road, E. Dulwich, S.E. 22.
M. Miller, E., M.A., M.R.C.S., L.R.C.P., 20, York Street, W. 1.
M.E. Miller, H. C., M.A., M.D., 146, Harley Street, W. 1.
Miller, Miss M, Education Offices, Margaret Street, Birmingham.
E. Millis, Miss M. K.
M. Mills, Prof., A. E., c/o Librarian, Fisher Library, University of Sydney Sydney, Australia.
M. Mitchell, T. W., M.D., Hope Cottage, Hadlow, Kent.
M. Monier Williams, M. S., M.A., M.R.C.S., L.R.C.P., 48, Onslow Gardens, S.W. 7.
E.I. Morant, G. M., 44, Pollards Hill North, Norbury, S.W.
Morgan, G. D., c/o, Miss Edgell, 15, Lyon Road, Harrow.
Morgan, Prof. G. Lloyd, LL.D., F.R.S.. 5, Victoria Square, Clifton, Bristol.
E. Morton, Miss M., M.A., 12, Tankerville Terrace, Newcastle-on-Tyne.
E. Mowat, Geo., M.A., B.Ed. 13, Wallace Street, Brucefield, Dunfermline, N.B.
M.E. Moxon, C., M.A., Box N, Los Altos, Cal., U.S.A.
Munday, Miss Rose E., The Old Rectory, Boughton, Stoke Ferry, Norfolk.
M. Munro, H. M. B., 11, Seymour Street, Portman Square, W. 1.
M. Murray-Lyon, T. M., M.D., 46, Palmerston Place, Edinburgh.
M. Murti, Capt. G. S., I.M.S., 1, Guindy Road, Adyar, Madras, S. India.
M.E.I. Muscio, Prof. B., M.A., The University, Sydney, N. S. Wales, Australia.
M.E.I.Ae. Myers, Dr. C. S., M.D., F.R.S., 2l, Portman Mansions, W. 1.
I. Myers, Mrs C. S., Winsford, Dulverton, Som.
E.. Neame, Barry, 9, Rectory Grove, S.W. 4.
E. Nettel, R., M.A., Pilgrims, Westerham, Kent.
E. Newcomb, Miss E. J., St Ives, East Grinstead, Sussex.
M.Ae. Newcombe, Miss M., M.A., The Red House, Market Harborough, Leicestershire.
M.E. Newman, Miss H. S., 52, Lower Sloane Street, S.W. 1.
E. Newton, Miss Hilda, Southlands College, Battersea, S.W. 11.
M. Nicoll, Maurice, M.D., 146, Harley Street, W. 1.
M. Nixon, J. C., M.B., Plympton House, Plympton, S. Devon.
M. Noble, Ralph A., M.B., Ch.M., "Wyoming," 175, Macquarie Street, Sydney, N.S. Wales, Australia.
Nodin, Miss Valentine D., 5, Garway Road, W. 2.
E. North, J., 20, Ridge Road, N. 21.
E.I. Nunn, Prof. T. P., M.A., D.Sc., London Day Training College. Southampton Row, W.C. 1.
E. Oates, D. W., M.A., L.C.P., "Aelyoryn," 13, Dewsland Park Road, Newport, Mon.
O'Donnel, S. P., I.C.S., Alderton, Simla, India.

	Ogden, C. K., M.A., 6, King's Parade, Cambridge.
E.	O'Neill, Edward E., B.A., Higher Dipl. Educ., 26, Cromwell Road, Belfast.
M.E.	O'Neill, Miss E. M., 18, Upstall Street, Myatt's Park, S.E. 5.
M.	Orr, D., M.D., County Asylum, Prestwich, Manchester.
	Osborne, C. H. C., Rendcomb College, Cirencester, Glos.
M.	Oswald, Miss V. O., London Neurological Clinic, Grosvenor Gardens.
M.	Otway, Rev. Arnold H., M.A., Trinity College, Cambridge.
E.	Overton, Miss F. M., 28, Dafforne Road, Upper Tooting, S.W. 17.
E.	Owen, John, M.A., Training College, Caerleon, Mon.
M.	Page, W. R., B.A., M.B., Ch.M.
M.	Pailthorpe, Dr Grace Winifred, M.B., B.S. (Durham), 40, Parliament Hill Mansions, N.W.
	Painter, G. C., B.A., 213 Galton Road, Warley Woods, Birmingham.
	Palmes, Rev. Fernando M., S.J., Ph.D., D.D., Prof. in Psychology, Laboratorio Psicologico del Collegio Maximo de S. Ignacio, Sarriá, Barcelona, Spain.
	Parker, Miss Evelyn M., B.A. (Oxon.), Craiglockhart R.C. Training College, Slateford, Edinburgh. (During July and August) Cherwell Edge, Oxford.
M.E.I.	Parsons, J. H., F.R.S., D.Sc., F.R.C.S., 54, Queen Anne Street, W. 1.
E.	Parulaker, R. V., M.A., Desai High School, Malvan, Ratnagiri, Bombay, India.
	Pasmore, E. S., M.D., M.R.C.P., Croydon Mental Hospital, Upper Warlingham, Surrey.
E.	Paton, W. B., B.Sc., 29, Florence Road, New Cross, S.E. 14.
M.	Payne, Dr Sylvia, C.B.E., M.B., B.S. (Lond.), 57, Carlisle Road, Eastbourne.
	Pear, Prof. T. H., B.Sc. 18, Chatham Grove, Withington, Manchester.
M.	Penrose, L. S., M.A., St John's College, Cambridge
M.	Peyer, Miss W., 3, Montague House, 194, Peckham Rye, S.E. 22.
E.	Peyton, Miss E., Littleton House School, Girton, Cambridge.
	Phelan, Rev. Dr. G. B., 174, rue de Tirlemont, Louvain, Belgium.
	Phillips, G. E., B.A., Teachers' College, Sydney, Australia.
	Philpott, Miss A. J., 19, Ostade Road, Upper Tulse Hill, S.W. 12.
E.	Philpott, S. J. F., B.Sc., Wick Drive, Wickford, Essex.
E.I.	Pickford, G. H., B.A., 33, Upper Rushton Road, Bradford.
	Pitt-Rivers, Capt. G., Manor House, Hinton St Mary, Dorset.
E.	Polishuk, Mrs P. L., 39, Fellows Road, N.W. 3.
	Pollard, Dr., 343, Hagley Road, Edgbaston, Birmingham.
E.	Porter, T. L. D., B.Sc., 162, Coventry Road, Ilford.
M.	Porter, Mrs S. C., Haughgate, Woodbridge, Suffolk.
	Pottinger, Rev. W., M.A., 17, Buccleuch Place, Edinburgh.
E.	Powell, Miss M. M., 40, Dunster Gardens, Brondesbury, N.W. 6.
M.	Power, T. D., M.D., D.P.M., D.P.H., Brentwood Mental Hospital, Essex.
	Pragasam, A. G., 21, Birchington Road, N.W. 6.
	Pragnell, Lady Leonora, "The Sanctuary," Washington, Sussex.
M.	Price, Miss L. E., 22, Bishopsthorpe Road, Sydenham, S.E. 26.
M.E.	Prideaux, E., M.R.C.S., L.R.C.P., Ministry of Pensions, Medical Service Division, 1 & 2, Sanctuary Buildings, Gt Smith Street, S.W. 1.
E.	Punnett, Miss, London Day Training College, Southampton Row, W.C.
I.	Quennell, F. T., Westfield House, Horbury, nr Wakefield.
E.	Quick, Miss, 32, Banbury Road, Oxford.
M.	Radclyffe, E. J. D., M.A., 37, Gt Ormonde Street, W.
M.	Rae, J. Burnett, M.D., Ch.B., 61, Harley Street, W. 1.
E.	Randle, Herbert Niel, M.A., c/o Messrs Thos. Cook & Son, Ludgate Circus, E.C. 4.
E.	Rawson, Miss H. L. C., B.A., City of Leeds Training College, Beckett Park, Leeds.
	Read, A. C., M.A., 8, Joubert Mansions, King's Road, Chelsea.
M.E.I.	Read, Prof. C., Woodlane, Birmingham Road, Solihull, Warwickshire.
M.	Read, C. S., M.D., 11, Weymouth Street, W. 1.

M.E.I. Reckitt, Miss E. C., 44, Buckingham Palace Mansions, Buckingham Palace Road, S.W. 1.
M. Rees, Dr J. R., M.A., M.D., 146, Harley Street, W. 1.
M.E.I. Reeve, L. R., 11, Crescent Way, Brockley, S.E. 4.
M. Reynell, W. R., M.D., M.R.C.P., 26, Nottingham Place, W. 1.
M.E. Richards, Miss G. A., 17, Albert Palace Mansions, Battersea Park, S.W. 11.
Richmond, K.
M. Rickman, John, M.A., M.B., B.Ch., 26, Devonshire Place, W. 1.
M.I. Riddell, Rev. John, M.A., B.D., 5, Town Moor Avenue, Doncaster.
M. Riddoch, G., M.D., 16, Devonshire Place, W. 1.
M.E. Riggall, R. M., M.R.C.S., L.R.C.P., 31, Wimpole Street, W. 1.
Ritchie, Miss Frances, B.Sc., Education Department, Birmingham University.
M.E.I.Ae. Rivett, Miss D. Mary, B.A., McIntosh Street, Gordon, Sydney, N.S.W., Australia.
M. Riviere, Mrs J., 10, Nottingham Terrace, N.W. 1.
M. Rivington, Miss E., M.R.C.S., L.R.C.P., M.B., B.S., 22, Crosfield Road, Hampstead, N.W. 3
E. Roberts, Miss A. M., M.A., 303, Ivydale Road, S.E. 15.
M.Ae. Roberts, Mrs Ursula, B.A., 19, Woburn Square, W.C. 1.
M. Robertson, Prof. G. M., M.D., Tipperlinn House, Morningside Place, Edinburgh.
Robertson, J. Boyd, Oakwood, Ashford, Kent.
M. Robinson, Prof. A., M.A., D.C.L., Observatory House, Durham.
M.E. Robinson, Miss Emily E., 3, Wolverton Gardens, Ealing, W. 5.
Robinson, Miss M. E., 1, Holmbury View, Upper Clapton, E. 5.
Robinson, W., M.B., D.P.M., Ch.B., Brentwood Mental Hospital, Brentwood, Essex.
Robson, A. H., M.Sc., 8, The Avenue, Watford, Herts.
E. Rodger, Mrs Robertson, "Ketches," Newick, Sussex.
E. Ross, Miss E. L. S., M.A., B.Ed., 9, Meadway Court, Meadway, N.W. 11.
M.E.I. Ross, G. R. T., M.A., D.Phil., 50, Marine Terrace, Aberystwyth. (Till June 30th.)
M. Ross, T. A., M.D., M.R.C.P.E., The Cottage, Swaylands, Penshurst, Kent.
I. Rowntree, B. S., J.P., The Cocoa Works, York.
Royle, Rev. R. H., Toc H., 71, Newhall Street, Birmingham.
M.E.I.Ae. Rusk, R. R., M.A., B.A., Ph.D., 14, Crown Gdns., Glasgow, W.
E. Russell, J. B., B.Sc., Southend House, Bury St Edmunds, Suffolk.
Ryecart, Miss C., 59, Newhall Street. Birmingham.
E. St George, Miss L. M., 121a, Gloucester Road, South Kensington, S.W. 7.
M. Salt, Miss S. M., 55, Circus Road Mansions, N.W. 8.
E. Salter, Miss H. de G., The Crown House, Newport, Essex.
Sampson, G. W., M.A., 74, Margaret Grove, Harborne, Birmingham.
M. Samuel, H. C., 428, Liverpool Road, N. 7.
Sargent, Major L. C., St Aidan's Lodge, Westgate-on-Sea.
M.E. Saunders, E. G., M.B., B.S., Crossways, Colin Deep Lane, Hendon, N.W. 4.
Saxby, Miss I. B., B.Sc., 21, Y Groes Rhubina, Cardiff.
Schiller, F. C. S., M.A., D.Sc., Corpus Christi College, Oxford.
M.E. Schiller, L. C., B.A., 59, Leinster Square, Bayswater, W. 2.
M.E.I. Scoresby-Jackson, Miss M., c/o Ladies National Club, 116, Westbourne Terrace, W. 2.
Scott, Miss E., B.A., Education Dept., The University, Edmund Street, Birmingham.
I. Scott Maxwell, Capt. J. M., B.Sc., M.I.E.E., Baillieston House, Baillieston, nr Glasgow.
M.E. Searl, Miss M. N., 16, Gordon Street, W.C. 1.
M.E.Ae. Seligman, Prof. C. G., M.D., F.R.S., Court Leys, Toot Baldon, Oxford.
I. Seligman, R., Ph.D., Lincoln House, Parkside, Wimbledon Common, S.W. 19.
Sen, Kenneth Kwei Ting, M.A., University of Amoy, Amoy, China.
M.E. Sennett, Miss M., 90a, Fielding Road, Bedford Park, W. 4.

E. Sergeant, Miss F. I., The Training College, Warrington, Lancashire.
M. Sergeant, Dr J. Noel, M.B., B.S. (Lond.), M.R.C.S., L.R.C.P., Newlands House, Tooting Bec Road, S.W.
Shakespear, W., 66, Dora Road, Smallheath, Birmingham.
M. Shand, A. F., M.A., 1, Edwardes Place, Kensington, W. 8.
Sharman, Miss E., C.S.M.M.G., L.A., 8, Ashburnham Mansion, Chelsea, S.W. 10.
M. Sharpe, Miss E., 16, Gordon Street, W.C. 1.
Sharpe, Mrs M. I., M.S.C., Tower House, 8, Candover Street, Mortimer Street, W. 1.
Shaw, G. B., 10, Adelphi Terrace, W.C.
E. Shearcroft, W. F. F., B.Sc., A.S.C., F.C.S., King's School, Peterborough.
E. Sheehan-Dare, Miss H. M., B.A., School for Girls, Battle Abbey, Sussex.
E. Sheppard, Miss Emily A., 62, Chudleigh Road, Brockley, S.E. 4.
E. Sheppard, W. F., Cadrona, Berkhampstead.
Sherrington, Prof. Sir C. S., G.B.E., M.D., P.R.S., The Physiological Laboratory, Oxford.
Shine, Rev. Prof., M.A., 24, St. Kevin's Park, Dartry Road, Dublin, Ireland.
M.E. Shrubsall, F. G., M.D., 15, Well Walk, Hampstead, N.W. 3.
Silsby, Miss Eleanor, B.Sc., Bedford House, Berkeley Gardens, W. 8.
Simitch, Z. V., B.A., 16, Leven Terrace, Edinburgh.
Simmins, Miss C. A., Collegiate Hall, Sheffield.
M. Simpson, F. G. McNeil, M.B., 7, Boar Lane, Leeds.
E. Sinclair, G. I., B.Sc., 113, Greerson Road, Honor Oak Park, S.E. 23.
Skelly, T. W. King, L.C.P., 30, Rose Hill, Harrington, Cumberland.
E. Sleight, W. G., D.Litt., 16, Eardley Road, Streatham, S.W. 16.
Slight, David, M.B., Ch.B., Combe Dept. of Psychology, University, Edinburgh.
E. Smith, Miss Agatha, Furzedown Training College, Welham Road, Mitcham Lane, S.W. 17.
E. Smith, Frank, M.A., University College, Aberystwyth.
M. Smith, Dr. G. W., M.B., Ch.B., O.B.E., Wyke House, Isleworth, Middlesex.
Smith, Dr. Hamblin, H.M. Prison, Birmingham.
Smith, J. W., 100, Grue Road, Sparkhill, Birmingham.
M.I. Smith, Miss M., 37, Abbey Gardens, St John's Wood, N.W. 8.
Smith, P. C. C.
E. Smith, Miss P. Woodham, M.A., Maria Grey Training College, Brondesbury, N.W. 6.
E. Smith, Rev. T. Grigg, M.A., 6, Birchfield's Avenue, Rusholme, Manchester.
Smith, W. Whateley, Borgfeld Lodge, Widmore Road, Bromley, Kent.
M. Snowden, E. N., M.B., M.R.C.S., L.R.C.P., 21, New Cavendish Street, W. 1.
Ae. Solomon, S. J., R.A., 18, Hyde Park Gate, S.W. 7.
E. Sowdon, E. W., B.Sc., 11, Kingsmead Close, Selly Oak, Birmingham.
Sowton, Miss S. C. M., Psychological Laboratory, Cambridge.
E.Ae. Sparling, Miss L. C., Municipal Training College, Cottingham Road, Hull.
M.E.I. Spearman, Prof. C., Ph.D., 71, Kensington Gardens Square, W. 2.
Ae. Speed, Harold.
E. Spence, Miss N., 30, Palace Court, W. 2.
E.I.Ae. Spielman, Miss Winifred, B.Sc., 29, Cambridge Square, W. 2.
M. Sprott, W. J. H., B.A., Psychological Laboratory, Cambridge.
M. Statham, H., M.B., B.Ch., M.R.C.S., L.R.C.P., 31, Christchurch Road, Bournemouth.
Ae. Stebbing, Miss L. S., M.A., 27, Belsize Park, N.W. 3.
E. Stephen, A. L., 50, Gordon Square, W.C. 1.
Stephenson, Arthur, B.Sc., University of London Club, 21, Gower Street, London, W.C. 1.
E. Stevenson, Miss E., 99b, Horseferry Road, S.W. 1.
E. Stewart, Rev. C. R. Shaw, M.A., 6, Queen's Elm Square, Chelsea, S.W. 3.
Stocks, J. L., M.A., D.S.O., St John's College, Oxford.

M.E.I.Ae. Stoddart, W. H. B., M.D., F.R.C.P., Harcourt House, Cavendish Square, W. 1.
M. Stoddart, Mrs W. H. B., Harcourt House, Cavendish Square, W. 1.
Stout, Prof. G. F., LL.D., F.B.A., Craigard, St Andrews, N.B.
Strachey, James, B.A., 41, Gordon Sq., W.C. 1.
M. Strauss, King's College Hospital.
E. Streeten, Mrs N., High Hilden, Tonbridge.
M.E. Streeter, Rev. Canon, M.A., D.D., Queen's College, Oxford.
Sturt, Miss M., B.A., 5, Park Terrace, Oxford.
E. Sutherland, I. L. G., Victoria University College, Wellington, New Zealand.
Swallow, Mrs C. B., 24, Eastoombe Avenue, Blackheath, S.E.
E. Sweeney, H. J., 3, Plowden Buildings, Middle Temple, E.C. 4.
E.I. Tagg, Max Herbert, B.Sc., A.K.S., F.C.S., 17, Hardwick Road, Chiswick, W. 4.
M. Tansley, A. G., M.A., F.R.S., Grove Cottage, Grantchester, Cambridge.
Tatlock, R. R., 19, Taviton Street, W.C. 1.
E. Taylor, Miss F. K., 44, Robinson Road, Tooting, S.W. 17.
M.E.Ae. Taylor, Miss N. G. R., Avery Hill College, Eltham, S.E. 9.
Taylor, Rev. T. W., M.A., Hon. C.F., The Clergy House, Church Stile, Rochdale.
E. Terry, Miss Ella M., 21, Manchester Street, Manchester Square, W. 1.
M.E.I. Thacker, C. R. A., M.A., M.B., B.Ch., Sidney Sussex College, Cambridge.
M.E. Thomas, Miss B., 9, Lansdowne Crescent, Cheltenham.
Thomas, Dr. G., 382, Bearwood Road, Smethwick.
Thomas, Rev. J. M. Lloyd, 47, Frederick Road, Edgbaston, Birmingham.
M.E.I. Thomas, Miss Maud Tuson, O.B.E., 2, Brondesbury Park, N.W. 6.
Thomason, Miss M. B., M.A., 87, Greenfield Road, Harborne, Birmingham.
E. Thompson, J. R., M.Sc., M.C., Education Dept., University, Sheffield.
E. Thomson, Miss Edith, M.A., B.Ed., 145, Dalkeith Road, Edinburgh.
M.E.I. Thomson, Prof. G. H., D.Sc., Ph.D., The Shiel, Moorside, Newcastle-on-Tyne.
M. Thomson, H. T., M.D., 13, Lansdowne Crescent, Edinburgh.
Thorburn, J. M., University College, Cardiff.
Ae. Thornton, Alfred H. R., The Poultry Court, Painswick, Glos.
Thorp, Sidney Benson, B.A., 44, Claygate Road, West Ealing, W. 13.
M. Thouless, R. H., M.A., Ph.D., Psychological Laboratory, University, Manchester.
E. Tibbey, T. G., M.A., 26, Addison Road, Bedford Park, W. 4.
Tighe, J. V. G. B., M.B., Stannington, Newcastle-on-Tyne.
E. Tilbrook, Miss J. M., 76, Upper Tollington Park, Stroud Green, N. 4.
M. Tippet, J. A., M.R.C.S., L.R.C.P.
M.Ae. Todd, R. W. L., M.B., Ch.B., B.Sc., Queen's Hospital, Sidcup, Kent.
Tolani, Prof. N. N., Indian Institute of Philosophy, Amainer, East Khandesh.
M. Tolputt, A., M.D., 93, The Headlands, Kettering.
M. Tolputt, Major P. T., L.R.C.S. & P. (Ed.).
E. Tompkins, A. G., Belvoir Cottage, Histon Road, Cambridge.
M. Townsend, Miss St Clair, North House, Northwood, Middlesex.
Trafford, C. R., J.P., B.A., Hill Court, Ross, Herefordshire.
E. Trivedi, H. D., Shri Daxinamurti, Vinaya Handir, Bhavangar, Kathiawar, India.
E. Trotter, T. H. Y., M.A., Mus.Doc., 7, St Alban's Mansions, Kensington Court, Kensington, W. 8.
E.I.Ae. Tucker, Mrs A. R. R., 24, West Street, Bridport, Dorset.
M. Tucker, D. Leslie, B.A., M.R.C.S., L.R.C.P., Warsop, Mansfield.
M.E. Turner, Miss J., B.A., 14, Endsleigh Street, W.C. 1.
M. Turton, Miss W., 18, Hanover House, Regent's Park, N.W. 1.
M.Ae. University of London Psychological Society, Sec., F. Akroyd, The Nook, Belmont Road, Bushey, Herts.
I.Ae. Urwick, Major L., O.B.E., M.C., M.A. (Oxon.), St Quentin, Great Malvern.

Ae.	Valentine, Prof. C. W., M.A., D.Phil., Greenland House, Greenland Road, Selly Park, Birmingham.
M.	Vance, Rev. J. G., B.A., Ph.D., St Edmund's College, Old Hall, Ware.
	Vaughan, Mrs. C., Courtfield, Ross, Herefordshire.
I.	Vernon, H. M., M.A., M.D., 5, Park Town, Oxford.
E.	Vickers, Wilfred, M.Sc., M.Ed., 73, Mustoka Drive, Bents Green, Sheffield.
	Vinter, Miss R., Torpoint, Cornwall.
	Vyle, G., Arncliffe, Cotton Lane, Moseley, Birmingham.
M.E.	Wakefield, Miss M. E., M.A., 58, Belsize Park Gardens, N.W. 3.
M.	Walby, Miss Rhoda, 14, Dawson Place, W. 2.
M.	Walker, Mrs N. S., 36, Rosary Gardens, S.W. 7.
	Wallas, Prof. G., M.A., 38, St Leonard's Terrace, Chelsea, S.W. 3.
E.	Wallis, L. F., B.Sc., F.G.S., Dunkery, Park Road, Barnet, Herts.
E.Ae.	Walston, Sir Charles, Litt.D., Ph.D., Newton Hall, Newton, Cambs.
E.	Walters, Miss Elsa H., B.A., 26, Clonmel Road, Teddington, Middlesex.
E.	Ward, H., Board of Education, Whitehall, S.W. 1.
M.E.I.	Ward, Prof. James, Sc.D., Litt.D., F.B.A., 6, Selwyn Gardens, Cambridge.
E.	Warth, Niss N , L.L.A., School for Girls, Battle Abbey, Sussex.
E.	Waterfall, Miss E. A., "Ashgrove," Trinity Road, Darlington.
	Watkins, J. P., "Winton," Temple Road, Croydon.
M.E.I.	Watkins, Prof. S. H., M.A., Phil.D., University College, Exeter.
M.	Watson, T. T. B., M.B., B.Ch. (Camb.), 78, George Street, Euston Road, W.
E.I.	Watt, H. J., D.Phil., Psychological Department, University of Glasgow.
E.	Watts, Capt. A. E., M.A., Peak Hotel, Hong-Kong.
	Watts, Frank, M.A., The University, Manchester.
E.	Watts, Mrs. J., 14, Windsor Court, Moscow Road, W. 2.
E.	Wayman, Mrs Mary, El Khan, Lytchett Road, Sundridge Park, Bromley, Kent.
E.	Webb, E., D.Sc., Berwyn, Totteridge, N.
M.E.I.	Weber, Miss H. M., Hadley Bourne, Barnet, Herts.
E.	Webley, T. J. B.A., The College, Caerleon, Mon.
E.	Webster, Miss J. H., 25, Empress Avenue, Manor Park.
	Wechsler, D., 1291, Maddison Avenue, New York, U.S.A.
E.I.	Welch, H. J., 1–4, Great Tower Street, E.C. 3.
M.	Welsh, Dr Sybil I., 105, Westbourne Terrace, W. 2.
M.E.	Wharton, Miss E. A., M.R.C.S., L.R.C.P., Withyham, Sussex.
M.E.I.	Wheeler, Miss O., D.Sc., The University, Manchester.
I.	Whipple, R. S., M.I.E.E., 15, Creighton Avenue, Muswell Hill, N. 10.
M.E.	Whitbread, W. B., M.A.
M.Ae.	White, H. D. Jennings, 9, Devonport Street, Hyde Park, W. 2.
E.	White, Mrs J., D.Sc., Auto-Education Institute, 93, Great Russell Street, W.C. 1.
M.	Whitehead, R., B.Sc., F.R.A.S., The Medical School, The University, Manchester.
M.	Whittle, C. H., B.A. (Cantab.), Addenbrooke's Hospital, Cambridge.
	Wigglesworth, Rev. H. E., B.A., B.D., L.R.A.M., 28, Elmore Road, Sheffield.
	Wilkinson, Miss E., 66, Witherford Way, Selly Oak, Birmingham.
M.E.	Williams, Dr A., D.P.H., M.R.C.S., 203, Albion Road, Stoke Newington, N. 16.
	Williams, Miss Beryl, Tanymarian, 34, Trinity Road, Llanelly.
	Williams, Rev. F. Paton, The Vicarage, Crawshawbooth, Manchester.
M.	Willoughby, R. M. P., LL.D., F.S.A., 97, Elm Park Gardens, S.W. 10.
M.	Wilson, A. C., M.R.C.S., L.R.C.P., 27, Nottingham Place, W. 1.
M.	Wilson, Arthur, M.B., 2, Comely Bank Road, Edinburgh.
I.	Wilson, D. R., M.A., Industrial Fatigue Research Board, 15, York Buildings, Adelphi, W.C. 2.
E.	Wilson, J. H., M.Ed., M.Sc., 39, Cambridge Road, Hove, Sussex.
	Wilson, S., M.D., 52, Newhall Street, Birmingham.

M. Wiltshire, Miss J., Ladies' Athenaeum Club, 7, Stratford Place, W. 7.
Wimms, J. H., M.A., B.Sc., 18, Woodriff Road, Leytonstone, N.E.
E. Winch, W. H., M.A., Melville Villa, Overhill Road, Dulwich, S.E. 22.
M. Wingfield, Hugh, M.D., 27, Welbeck Street, W. 1.
M.E.I. Wohlgemuth, A., D.Sc., Rutland Lodge, May's Hill Road, Shortlands, Kent.
Wolf, Prof. A., M.A., D.Litt., 57, Purley Road, Wandsworth Common, S.W 12.
M.E. Wolters, A. W., M.A., University College, Reading.
E. Wood, Miss M. H., The Cambridge Training College for Women, Wollaston Road, Cambridge.
M. Woodcock, O. M., M.D., 78, Coldharbour Road, Redland, Bristol.
Woodhull, Miss Z. M., Narton Park, nr Tewkesbury, Worcestershire.
M.E. Woods, Miss A., St Ives, Cross Path, Radlett, Herts.
E. Wright, G. G. N., M.A., B.Ed., Dept. of Adult Education, University College, Nottingham.
M. Wright, M., M.D., 86, Brook Street, Grosvenor Square, W.
M. Wrinch, Miss D. M., Girton College, Cambridge.
Wyatt, S., M.Ed., Tudor Cottage, Bramwell Lane, Davenport, nr Stockport.
E.I. Wynn-Jones, Ll., M.A., Ph.D., Psychological Department, University of Leeds.
M. Yellowlees, D., M.B., Ch.B., 5, St. James' Terrace, Glasgow, W.
E. Young, Miss M., M.A., B.Ed., 11, Findhorn Place, Edinburgh.
E. Yule, G. Udny, M.A., F.R.S., St John's College, Cambridge.

BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the Meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of members.

12. Every Candidate for election to the Society shall be recommended by at least two members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from Membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to sub-divide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall

immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, Chairman and Secretary of each of the Sections and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of members to serve on the Council may be suggested by the Council or by any member by sending the proposed names to the secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by ordinary members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent members, to whom the resolutions adopted shall be forwarded.

29. The first meeting of each Special Section shall be convened by the General Secretary of the Society. At this meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall

be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or failing him a Vice-President, shall whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all members of the Society notices of each meeting at least seven days beforehand, and of the business to be transacted at the meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty members; at least fourteen days' notice of such meeting shall be sent to each member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any meeting. The Council or a Sectional Committee shall however have the power of closing any meeting or meetings to visitors, provided notice to this effect be given on the notice convening such meeting or meetings. The names of visitors together with the names of members introducing them shall be entered in a book provided for that purpose. Any member of the Society attending a meeting of a section to which he does not belong must be introduced by a member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of ordinary members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as ordinary members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

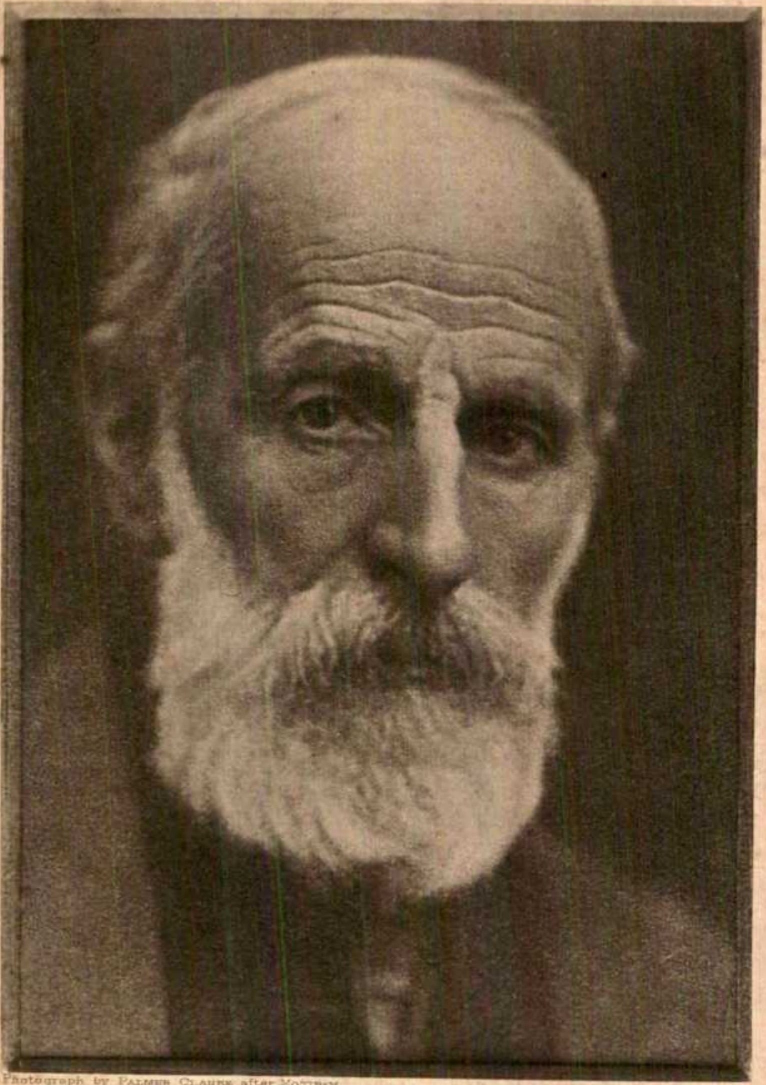
46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.



Photograph by Palmer Clarke after Motiram

James Ward 1843-1925

VOLUME XVI JULY, 1925 PART 1

THE BRITISH JOURNAL OF PSYCHOLOGY GENERAL SECTION

PROFESSOR JAMES WARD

1843–1925.

BY W. E. JOHNSON.

IN the death of Dr JAMES WARD the world has lost one of the most distinguished and influential philosophers of the day. Though primarily known as a psychologist he will be equally widely remembered as an exponent of Idealistic Philosophy who appealed to a wider class of thinkers perhaps than any other writer of the century. For unlike Bradley his system was constructive and in place of merely negative onslaughts upon science and religion Dr Ward applied his comprehensive knowledge of the one and his profound concern with the other to build up a philosophy which went far to meet the needs of all serious thinkers.

Dr Ward was a voracious reader and a trenchant critic. He aimed at encyclopaedic knowledge and was ready to dispute the theories of specialists in almost every department of learning. His early training in physiology was of service in tackling Weissmann when the views of that great champion of the non-inheritance of acquired capacities were in vogue. When the physicists were reducing all knowable phenomena to what could be subsumed under the principle of the Conservation of Energy, Ward forcibly disputed the scientific adequacy of their position. And in his latter days he entered into the contest that has been raging upon the epistemological and physical questions raised by Einstein's Theory of Relativity.

Perhaps some reminiscences of my own of Ward's influence as a teacher of psychology will be interesting to readers of the present day, many of whom may have forgotten the problems which confronted us in the late seventies and early eighties. I first became his pupil in 1880, when Bain and Spencer were almost the only representatives of English Psychology, when Sully was becoming known as an original contributor to psychological thought with an equipment of German knowledge, when

William James was hardly heard of, when Helmholtz, Fechner and Lotze were still living and dominated respectively the physiological, the psychophysical and the metaphysical sides of psychological speculation. How eagerly we read the controversies on Colour and Sound sensations in which numberless original investigators were taking sides between Helmholtz and Hering, or were proffering intermediate theories which sought to bring all the apparently conflicting facts of persistence, contrast and fusion of sensational processes into harmony! It was Ward whose critical and expository powers gave light and heat to these discussions among us. Then again the great psychophysical formula erected by Fechner upon the first crude experimentations of Weber was being variously re-interpreted and finally fairly overthrown; and here again it was Ward who taught us to mingle caution with zeal in investigating these problems. But questions of even greater profundity were raised by Lotze whose metaphysical, logical and psychological speculations found in Ward the most critical and yet sympathetic exponent. In particular the local-sign theory of space perception was re-shaped and more firmly established by Ward's notion of the presentation-continuum which brought together many previously unconnected speculations into unexpected unity.

In all these controversies Ward's methods contrasted with those of his great contemporary William James. The latter had no adequately comprehensive view of the problems of philosophy and psychology, and was apt to be led away by his own imaginative and graphic powers to adopt one-sided views which, when comparing his conclusions on different issues, were impossible to reconcile. But Ward looked upon all the problems of philosophy as a whole. If as a Metaphysician he opposed every materialistic interpretation of reality, equally as a Psychologist did he reject the physiological explanations of conscious experience so much favoured by James. Both these great thinkers strongly maintained the Freedom of the Will; but James (first under the influence of Pierce and Bergson) adopted a fanciful and hardly intelligible form of Pragmatism which Ward strenuously opposed. I think Ward would have subscribed to the view which attributes causal efficiency to the human will as *the* essential factor in moral and material evolution, while ready to admit a non-materialistic form of determinism.

Another thinker whose influence over Ward should be mentioned is Herbart. Following Herbart in his rejection of the Faculty Psychology, Ward nevertheless strongly opposed the Herbartian tendency to treat the whole question of cognitive development merely as a matter of the mechanical interaction of presentations. It was here that Ward brought into use his insistence on the reality of the *Subject*, as an empirical and not merely transcendental agent. According to him the arousal and conflict of presentations, their combinations and groupings cannot be expressed in the language of dynamics which implies that the form of the whole is determined solely by the nature of the parts. Intelligence and interest have their origin elsewhere than in the manifold of presentations; and it is the function of these factors to control the development and determine the ultimate form assumed by the presentational whole.

Next to Lotze, the philosopher with whom Dr Ward was probably most in sympathy was Kant, and at the very end of his life he published a work which will hold its place among the most valuable critical expositions of that philosopher. In fact, as it seems to me, the originality of Dr Ward's treatment of psychology was due to his utilising the fundamental principles of the Critical Philosophy to give a 'rational' foundation for the science. Thus he incorporated along with the merely empirical data of psychological theory, a recognition of the Kantian Categories, substantially reformulated and interpreted anew, and thereby incidentally proved the possibility of a rational psychology which Kant had rejected.

Is it possible to trace the *motif* running through Ward's multitudinous contributions to thought? At first sight, it might appear that he worked at different topics on independent lines. For him, epistemology, ontology and physical science were to be treated each from its own standpoint. And yet, as it seems to the present writer, the clue to his thought is to be found in the unification of these different standpoints. Thus, his insistence—in opposition, for instance, to Bradley—on the conception of the *Subject* meant that for him Psychology was not merely an empirical science. The duality of subject and object, which is now very universally accepted almost in the sense laid down by Ward, was as much a basis for his Epistemology as for his Psychology. When upon this duality he based his denial of dualism, it is difficult to say whether his epistemology dominated his ontology or his ontology dominated his epistemology. The very important conception of Subjective Selection in antithesis to Natural Selection, although, on its first introduction seeming to concern Psychology alone, yet entered later as an essential factor in his very daring speculations in Biology. On the whole, I think we must say that his specific psychological theories profoundly influenced his position on all philosophical and scientific questions. As regards psychology he gave a second place to experimental and psycho-physical work; although he did more than anyone in England to develop the study of these branches in the colleges and universities of his country. Psychogenesis again was subordinated to what I should have called Psycho-analysis—had not that term been wantonly introduced with a restricted connotation. In opposition to the modern tendencies or exaggerations of Behaviourism, his work remains of the greatest value in bringing us back to the necessity of what must be termed Introspection. So far, however, as introspection means the examination of what is special to a person or to his transitory mental processes, Dr Ward attached very little value to this line of enquiry, and was not much in the habit of indulging in the practice himself. The analysis of mental processes which Ward encouraged was rather directed to the generalised forms of psychosis than to the variable content which might distinguish this or that actual psychosis from another. Thus the purpose of his examination of the relations between sensation, cognition, belief, feeling, conation and action—as they enter together in constituting a psychic unity—was to reveal the ultimately real nature of Experience itself. It was in this sense, that his psychological work constituted a contribution to Ontology, to Epistemology and even to Ethics. Viewing experience in this light, the problem of Theism, the significance of values, and above all the supremacy of ends were brought into relation in the construction of a system of Philosophy which might be termed eminently Humanistic.

The above sketch conveys, I hope, even to those who had no personal acquaintance with him, some sense of the searching thoroughness and consistent sincerity of this great seeker after truth.

THE CAUSES OF THE CONTINUOUS CHANGE OF RESISTANCE OBSERVED IN PSYCHO-GALVANIC EXPERIMENTS

BY ROBERT H. THOULESS.

(*From the Manchester Psychological Laboratory.*)

IT is found, that, in the course of experiments on the psycho-galvanic phenomenon, there is a continuous change in the subject's resistance in addition to the rapid discontinuous changes resulting from emotional stimulation. About this continuous change, no satisfactory information is to be found in the literature of the psycho-galvanic phenomenon. Guilleminot[1], for example, states that there is a constant decrease of resistance due to the passage of ions of the electrolyte into the skin; Veraguth[2], on the other hand, says that there is a constant increase, which is attributed by Prideaux to the increasing polarisation of the skin[3].

The truth appears to be that, under the same conditions, the resistance of some subjects increases and that of others decreases; and it can be shown that the decrease is not mainly due to the penetration of ions, or the increase to increased skin polarisation (although both of these factors may have some effect on the phenomenon).

First Series of Experiments.

The essentials of my electrical arrangements have been the same as those most commonly used by other workers on this subject. The connections are shown on Fig. 1. The external current was supplied from a single-cell accumulator. A mirror galvanometer was used with a universal shunt. The electrodes were made of sheet zinc (afterwards of a flat coil of silver wire covered with silver chloride), and enclosed in chamois-leather bags soaked in a solution of sodium chloride. A two-way switch was introduced into the circuit which made it possible to cut out the accumulator and replace it by a short-circuit. The object of this switch was to make it possible at any moment to measure directly the effect of polarisation on the apparent resistance of the subject. Since the resistance of the accumulator is negligible compared with the other resistances of the circuit, the effect of replacing it by a short-circuit is to leave the resistance-net unchanged in all respects, the only alteration in the circuit being the cessation of the external current[1]. Therefore, the

---

[1] *Handbook of Electricity in Medicine*, New York and London, 1906.

[2] *Das psychogalvanische Reflexphänomen*, Berlin, 1909.

[3] "The Psychogalvanic Reflex: a Review," *Brain*, vol. XLIII, 1920–21.

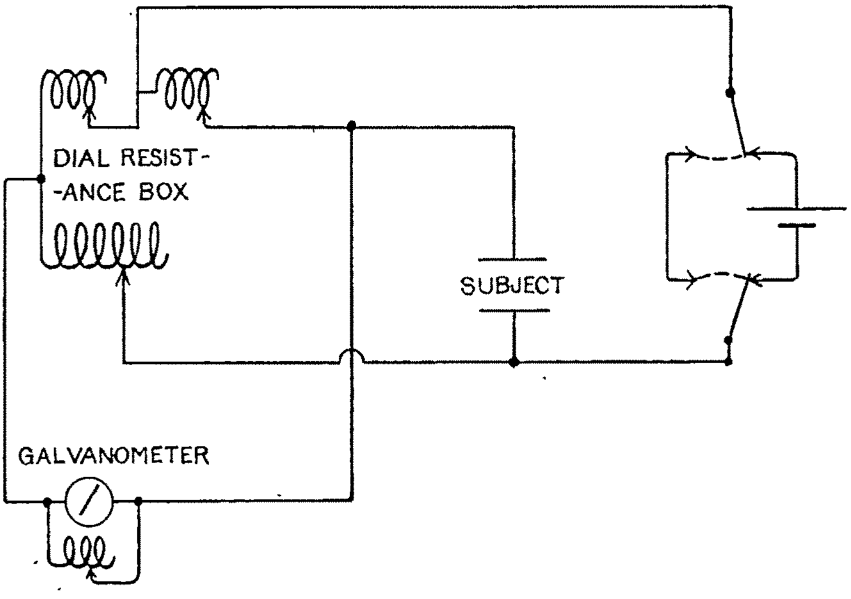


Fig. 1.

---

[1] It would, of course, be possible to calculate from the deflection produced by the polarisation current, its actual voltage. This, however, is of no particular interest for the purpose of the present experiment. What we require to know is its effect on the apparent resistance of the subject. The advantage of the connections given in Fig. 1 is that this arrangement enables the effect of polarisation on the observed resistance to be measured with the greatest ease.

An example may serve to make this method clear. At the end of the lowest curve shown in Fig. 5, the subject *X* was found to have a resistance of 10,900 ohms. The coils of the dial resistance box were at 100 ohms, 1,000 ohms and 1,090 ohms. As soon as the spot of light had been brought to zero, by the adjustment of the variable resistance to 1,090 ohms, the switch was put over to the position in which the accumulator was out of circuit and was replaced by the short-circuit. The galvanometer then gave a deflection of – 6 mm. Previous measurement with a variable standard resistance in place of the subject had shown that a deflection of – 6 mm. was given by a resistance of 10,950 ohms (when the resistances of the dial bridge were 100ω, 1,000ω and 1,090ω). The polarisation current was, therefore, making the subject's resistance appear to be greater than its actual value by 50 ohms. A correction of – 50 ohms had to be applied to the observed resistance in order to obtain its true value.

It is hardly necessary to point out how small is this value as compared with the observed variations in resistance. That polarisation changes should ever have been used to account for these can only have been due to observers neglecting altogether to measure the actual value of the polarisation effects.

deflection recorded by the galvanometer when the switch is suddenly turned over from the cell to the short-circuit is a direct measure of the amount of the effect of polarisation (in the electrodes and the subject's skin) on the deflection previously obtained when the cell was in circuit. In order to keep the subject's distribution of attention as uniform as possible, he was given a watch with a seconds-hand and instructed to tap once at the end of every ten seconds and twice at the end of each minute.

A simple experiment disproving the views both of Guilleminot and of Prideaux on the causation of the continuous change in resistance is suggested by the fact that both are represented as effects of the passage of an electric current. If this were true, the change should not take place in the absence of an external current. I measured, therefore, the continuous change in resistance under two conditions: (*a*) when a current was flowing continuously through the subject, and (*b*) when a current was flowing only during the few seconds necessary to measure the subject's resistance by balancing the dial bridge. It is true that in the second class of observations, the current is flowing through the subject for a short time, but if the resistance change is a direct result of the passage of the current, this change should be much less rapid in the second case than in the first. Fig. 2 shows curves from subject *Y* with and without a continuous current. Fig. 3 shows two curves obtained respectively by

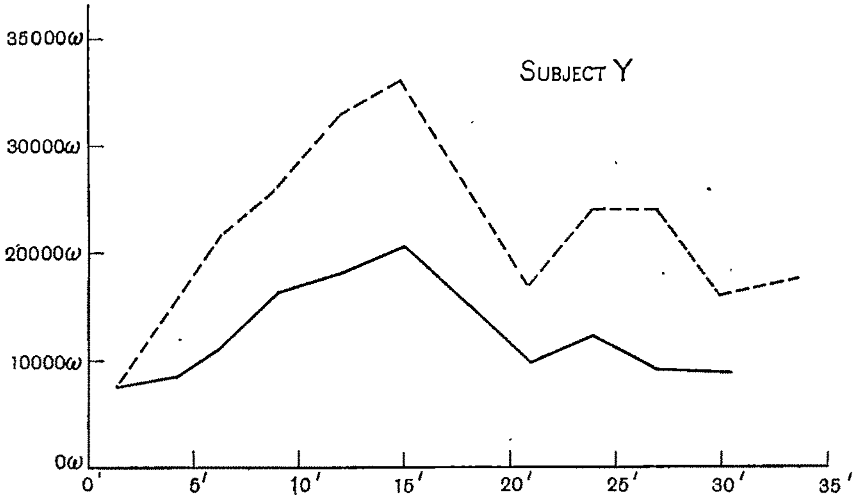


Fig. 2.

——— resistance curve with continuous current
- - - - ,, ,, without continuous current.

combining four curves of *X* with a continuous current, and by combining three curves obtained with no continuous current. It will be seen that the curves of *Y* show increasing resistance, while those of *X* show decreasing resistance, quite independently of whether a current has been passed continuously through the body or not. The phenomenon is, therefore, in the main, due to no effect of the passage of the current. Polarisation in the one case, and the passage of ions through the skin in the other, are thus eliminated as prime causers of the effect.

The fact that increasing polarisation is not an important factor in the case of increasing resistance can, moreover, be settled in a more direct manner, for it is possible by means of the two-way switch to interrupt the current at any moment and measure the deflection due to the total back E.M.F. (from both electrodes and subject) due to polarisation. This is found to be an amount so much smaller than the increases of resistance observed that its effect may clearly be discounted. For example, when a continuous current was passed through subject *Y*, the total increase of his resistance to its maximum at the end of fifteen minutes was from 7,700 ohms to 20,500 ohms, but the component of this apparent increase due to increased polarisation was only of the order of 100 ohms. It seems difficult to avoid the conclusion that Prideaux's attribution of the increased resistance effect to the subject's polarisation was due to his ignoring of the actual magnitude of the polarisation effect and of its effect on the apparent resistance. Actual measurement shows that the increase in resistance is enormously greater than can be accounted for by the measured polarisation effects.

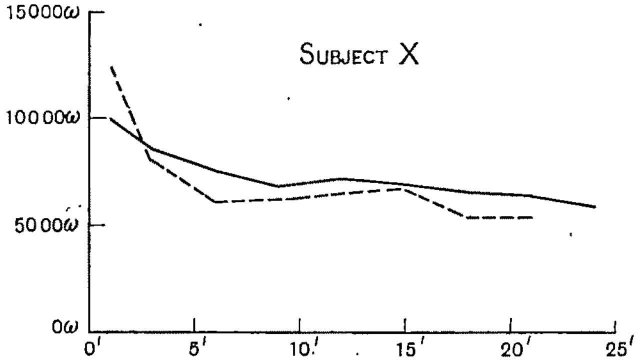


Fig. 3.

——— mean resistance curve with continuous current

- - - - „ „ „ without continuous current.

A further fact which suggests that a very important factor in the production of these changes is not a peripheral physical one at all is the observation that the subject's resistance dropped at once to a permanently lower level when I gave the warning: "Now I am going to start firing the pistol[1]." If the subject's resistance was high before I gave the warning the drop in his resistance level was proportionately great. For example, subject *Y*, in one observation (not shown on the diagram) attained the resistance of 48,000 ohms at the end of fifteen minutes. The threat of the pistol drove it down to 14,000 ohms, and it remained oscillating about that level while the pistol was being fired. After the curve shown in Fig. 3, *Y*'s resistance dropped from 17,500 to 6,400; after the Fig. 2 curve from 8,800 to 6,600. *X*, whose total resistances were smaller, dropped by a smaller amount on the threat of the pistol but generally to a lower level than *Y*. The following are *X*'s drops: 3,690 to 3,000, 8,200 to 6,900, 4,700 to 4,600, 7,200 to 6,500. Once, the firing of the pistol was begun before the beginning of the measurement of the subject's resistance, and it was then found that the resistance curve started on a low level with only a slight tendency to rise.

It is clear that the only one of the conditions of the experiment which has changed after the pistol warning has been given is the mental adjustment of the subject. Yet these drops were sometimes enormous, as, for example, in the 1st expt. with *Y*, where his resistance dropped from 48,000 to 14,000 ohms[2]. Taken in conjunction with the fact that the subject's resistance rises as he passes into the condition of sleep, we seem to have sufficient evidence (in the inadequacy of their suggested physical explanations) for supposing that the initial rises and falls of the subject's resistance are also due (at least, in part) to a change in his mental adjustment. *The condition of low resistance is apparently a condition of preparedness to react to a stimulus*[3].

A lowering resistance from the beginning of the observation means that the subject is adapting himself to the requirements of the experiment[4]. The difference between the curves of *Y* and *X* was possibly related to a characteristic difference in their mental attitudes towards the experiment. *X* was a laboratory steward whose attitude towards his part from the moment of beginning was one of earnest conscientiousness. *Y* was a research student who probably did not treat my requirements with the seriousness of *X*, and, indeed, reported after one of the experiments that dream-images were floating through his mind at one stage (possibly somewhere about the point of maximum resistance at 15 minutes).

---

[1] These observations were made during the course of a research in which, after studying the drift of resistance for about half an hour, I used the explosion of a toy pistol as stimulus for causing sudden resistance changes.

[2] It is necessary to make it clear that what I am describing here is a drop of the general level of resistance to a new level. This is quite different from the resistance change following the pistol-shot. This was a sudden lowering of resistance followed by a slow rise, and it was never as big as the drop here recorded—being generally from 10–15 per cent. of the total resistance of the subject.

[3] The word 'alertness' has been suggested for this condition of preparedness.

[4] See note at end.

Second Series of Experiments.

In order to show by another method that these resistance changes were independent of polarisation phenomena, observations were made of the resistance change which took place when the current used was alternating[1].

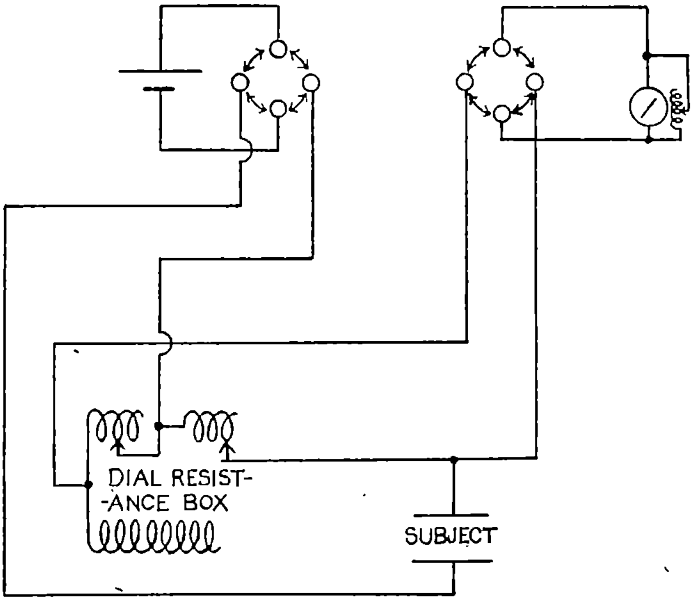


Fig. 4.

The connections used for this purpose are shown in Fig. 4. The reversal of the current took place by means of an electrically driven double commutator, which was rotated at the rate of about 100 revs. per minute. It will be seen that the connections are such that the current is alternating through the whole circuit but the direction of the galvanometer connection is reversed simultaneously so that the current is flowing always in the same direction through the galvanometer. This connection is preferable for this purpose to the simpler one in which it is the connection of the subject alone which is reversed, for in the latter arrangement it is necessary to make a correction in the observed resistance to allow for the fact that his apparent resistance is altered by the short time between the two directions of flow of the current during which it is not flowing at all. In the arrangement I adopted, this factor does not alter the point of balance, and does not therefore affect the subject's apparent resistance.

---

[1] These experiments were suggested by Prof. E. V. Appleton, King's College, London.

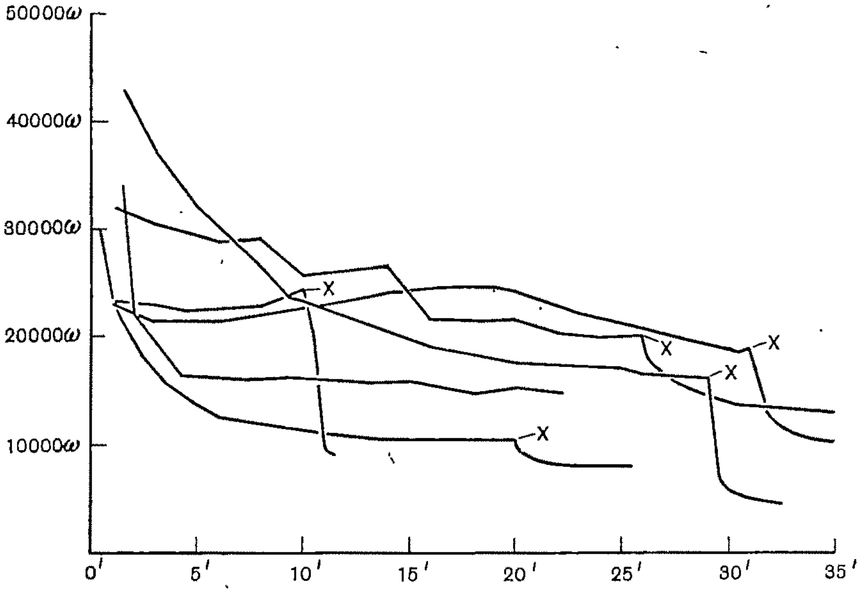


Fig. 5.

Application of this method to the subject X produced a descending curve of the same general form as before, and the same drop as before was obtained when the pistol warning was given. Unfortunately I was unable to make any new observations on Y since he had left England, and I have not been able to discover any other subject whose resistance rises after attaching him to the apparatus. The method was applied to many other subjects, however, all of whom showed a continuous fall in resistance, although the rate of fall was different for different subjects. Six of the curves so obtained are shown in Fig. 5. In all of these except one the drop in the level of resistance when the pistol warning was given and the pistol afterwards fired is shown by the change in level of the curve after the point marked X[1].

It will be seen that these curves confirm what had been established in the previous series of experiments. There is a continuous fall in resistance with a sudden drop when the subject's alertness is increased by the pistol warning. If the resistance was already low when this new demand on the subject's alertness is made, the consequent drop is, on the whole, less.

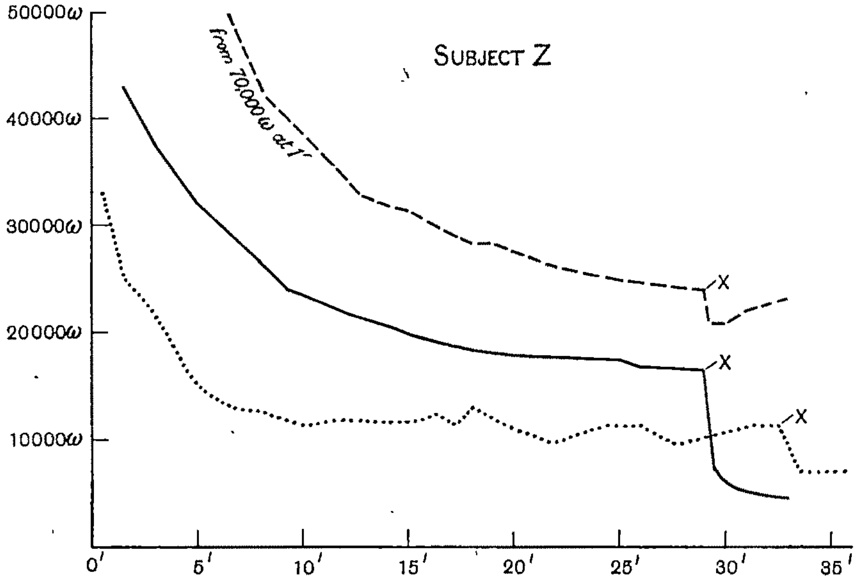


Fig. 6.

——— alternating current
– – – – direct current (– ve to palm)
.... „ „ (+ ve to palm).

At this point, a disturbing observation was made which threw doubt on the adequacy of the first series of experiments. I found that after the subject's resistance had attained a fairly constant level after an alternating current had been passed through him for about half an hour, further changes occurred in his resistance if a direct current was passed through him. This change in resistance varied with the direction of the incoming current, being a decrease if the positive electrode was placed on the palm of the hand, and an increase if the negative electrode was on the palm[1].

---

[1] It should be noted that in all the diagrams illustrating this article, the actual form of the curve after the pistol warning was given is not shown. This is, in fact, very complicated for there are rapid drops in resistance followed by a partial recovery, which could only be adequately recorded by a photographic method. After the point X, the curves merely show the changed level of resistance from which these sudden drops start.

This observation suggested that there might be no significant difference between the resistance drifts of *X* and *Y* but that I had merely chanced to connect *Y* with the negative electrode to his palm while I had always connected *X* in the opposite way. This explanation seemed improbable, for I had made three observations with *Y* and seven with *X*, and, moreover, if this were the true explanation of the earlier results it would be difficult to account for the fact that both rise and fall took place when a current was not passing continuously through the subjects.

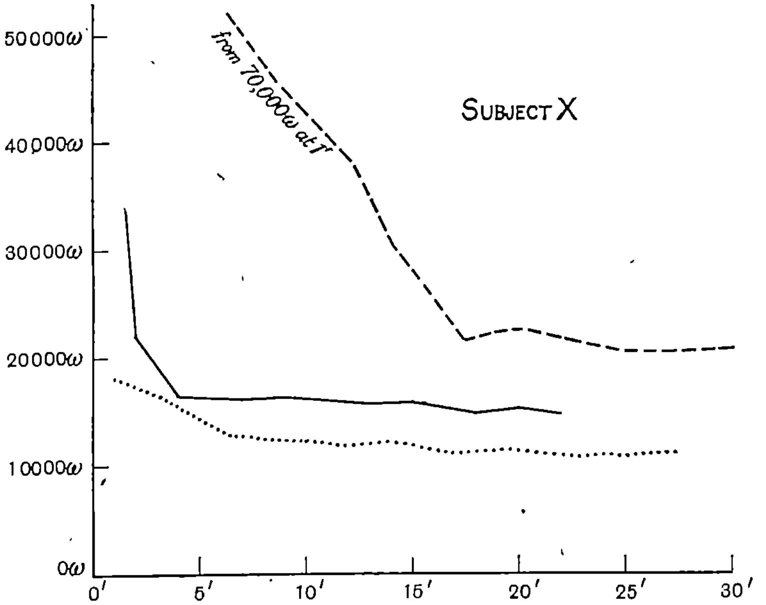


Fig. 7.

——— alternating current
- - - - direct current (+ve to back of hand, −ve to palm)
.... „ „ (−ve „ „ +ve „).

In order to be certain on this matter it was necessary to make new observations, comparing the resistance drifts obtained with both directions of passage of the current and that obtained with an alternating current. This was done with two subjects *X* and *Z* (*Y*, as has already been said, was no longer available). The curves obtained are shown in Figs. 6 and 7.

---

[1] This reversal itself seems to be a fact difficult of physical explanation. That the increased resistance is not due to polarisation can be proved in the manner already described.

These figures show that although the general level of the curve is different under these different conditions its general form is not altered[1].

When, therefore, a direct current is used there is a factor in the curve obtained, which is physical in its nature (*i.e.* independent of the subject's alertness) which is dependent on the direction of the current. Whether there is also a physical component in the downward drift generally obtained with an alternating current, I am not able to say. The usualness of a rapid decrease at the beginning of observations with many different subjects leads one to suspect that there is. If there is, these experiments show that it is not due to the passage of the current itself, but it may be due to soaking of the skin by the electrolyte. The experiments with *Y* show clearly that if there is such an effect it may be masked altogether by a change in the subject's alertness.

A further point was investigated with the subject *Z*. Drops on the resistance level had always been obtained by the threat of the discharge of the pistol. It seemed desirable to discover whether a similar change would take place if the subject were given a task needing concentrated attention but which would not produce the slight affect of fear which is certainly present while waiting for the firing of the pistol.

For this purpose, the subject *Z* was given the 'aiming test' at the point marked *X* on the dotted line of Fig. 6. His resistance dropped from over 11,000 ohms to 7,000 ohms, in the same way as it did when I gave the more usual pistol warning. On the day when I took the record shown by a broken line on the same diagram, the subject was given the 'reaction-time experiment.' In this case, a much smaller drop was observed. This was apparently due to a difference in the subject himself on that day and not in the demands of the experiment[2], for he was then asked to carry out the 'aiming test' as before and showed no further drop in resistance.

[1] In both of these figures, it will be noticed that the curve in which the negative electrode is on the subject's palm, is at the lowest level and shows the least change. If this is found to be always true, it is probably the best connection to use for experiments on the psycho-galvanic reflex, in which the drift of the subject's resistance is merely a nuisance to the experimenter because it necessitates continual change of the variable resistance.

It should be noticed that these observations do not mean that the resistance of a subject at any moment would appear different if measured by means of a current in one direction from the resistance obtained if it were measured by a current in the opposite direction. His resistance *is* different (however measured) if a current has previously been flowing from the palm to the back of the hand from the resistance he would have if the current had been flowing in the opposite way. This is shown by the fact that if the direction of current is reversed, the new level of resistance is attained only slowly.

[2] On this day, *Z* spontaneously informed me that he expected his curve would be irregular for he had been 'upset' that morning.

CONCLUSIONS.

1. The steady change in the subject's resistance observed in psychogalvanic experiments may be in either direction, although most subjects show a fall in resistance.

2. The level of the resistance curve is in part dependent on the direction in which current is passed through the subject.

3. The change in resistance is due neither to increasing polarisation nor to penetration of the skin by ions.

4. The main effect is due to a change in electrical resistance. While physical factors may also be present, I suggest that one very important factor in the change of resistance is the mental adjustment of the subject. The condition of low resistance appears to be one of preparedness to react to a stimulus (*alertness*), while the condition of high resistance is one of unpreparedness to react (a condition which culminates in the state of sleep).

Note. Since writing the first part of this paper, further observations have led me to believe that there is a physically caused lowering of resistance after attaching the subject to the apparatus. While *Y*'s rise in resistance, and both subjects' fall in level when the pistol was threatened were, therefore, due to change in alertness; *X*'s slow fall was not necessarily so. Further research is necessary to determine precisely the magnitude of this physical factor so that it may be discriminated from the effect of change in alertness.

(*Manuscript received* 2 *January* 1925.)

FEELING, IMAGING AND THINKING

By F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

§ 1. *The general conditions of feeling* (pp. 16–18).
§ 2. *Affect arising from the conflict of tendencies* (pp. 18, 19).
§ 3. *Affect arising from imperfect discrimination* (pp. 19–22).
§ 4. *Diffusion of affect* (pp. 22, 23).
§ 5. *Transference of affect* (pp. 23, 24).
§ 6. *The functions of affect* (pp. 24, 25).
§ 7. *Feeling and imaging* (pp. 25, 26).
§ 8. *Feeling, imaging and thinking* (pp. 26, 27).
§ 9. *The pleasurable feelings* (pp. 27, 28).

§ 1. THE GENERAL CONDITIONS OF FEELING.

ALTHOUGH perhaps nothing has been more discussed in recent psychology than the problems of affect, yet the exact functions of feeling in mental development and the relations of these to the functions of imaging and thinking are still far from clear. It is however obviously important that they should be investigated. Not only in systematic psychology, but constantly in everyday observation, it has been pointed out that while affect and imaging seem frequently to be found together, as if the one may stimulate the other, feeling and thinking are, in a general sense, antagonistic. In this paper I propose to attempt to show why this is the case. It is necessary to begin with a consideration of the general conditions under which affect, in its varied forms, may arise.

One method of approach is to ask what are the outstanding characteristics of those mental responses which are freest of all from affect, or what are the temperamental characters of those persons who appear to be least affective in their normal mental life. There are two. Whenever there is adaptive, or purposive, behaviour which is carried out immediately and successfully upon the presentation of a situation to which response is demanded, there is no affect, or very little. And secondly, whenever the cognitive material defining the situation to which a response is made—whether it be perceptual, imaginal, or ideal—appears clearly and sharply defined at the time the response is being made, there is no affect, or very little. Contrariwise, whenever (*a*) a response is held up, obstructed in some way, or (*b*) the cognitive material involved in the making of a response is blurred, vague or indefinite, affect tends to appear.

Let an object be presented with which we are very familiar, something that has been presented many times already, and let recognition be immediate, then, unless the recognition is linked with other reactions in a series which forms a single 'situation[1],' there is no apparent affect. This happens to everybody many times every day. For example, I come each morning into the room in which I am now writing, and without a trace of hesitation I respond perceptually to a number of objects. I notice them, and then proceed to some other task. There is no marked affect in the perceptual recognition, and unless we are concerned to maintain a barren logical theory about the necessity for every response whatsoever to be accompanied by pleasure-pain, I do not see that we need admit any affect at all. Again, suppose I take up my pen and begin to copy a manuscript. If the ink runs well, the pen works smoothly, and my writing musculature is in good order, the act of copying as such proceeds without affect. There is not even an affect of satisfaction unless the various writing movements are looked upon as related in a series which is progressing somewhere, and the progress towards the end somehow enters into my experience of the fact of the writing. Finally let me be *stating*[2] an argument. Let me see all my way perfectly clearly from the beginning to the end. Let the words, the images, the phrases and the whole course of them be plain before me, and once more there is a minimum of affect.

These are common observations that everybody has made. The 'unemotional man' of daily terminology, the 'unaffective' man as we perhaps ought to call him, is either the man of supreme practical effectiveness within a limited sphere, or the man of remarkable intellectual clarity. No doubt as soon as statements of this sort are made, striking exceptions[3] come to our mind. But in general the statements are true, and for the present the exceptions may be allowed to stand over.

Such observations might appear to lead immediately to the view that the only condition for the occurrence of affect is conflict among different reaction tendencies. But this cannot be accepted without the most careful scrutiny.

---

[1] 'Situation' is no doubt a term which is much in need of definition. From my point of view a 'situation' is all the cognitive, affective and reactive factors which are, at a given time, the limits of which must be determined empirically, organised by a common interest.

[2] The situation is very different when an argument is being not merely *stated*, but constructed.

[3] It is possible, for example, to regard 'apathy' as falling outside this formulation.

In the first place it appears certain that such conflict may occur and yet no evidence of affect supervene. Adaptive behaviour seems in fact to belong to every grade of organic reaction, and to be present long before we have the slightest justification for speaking of the presence of awareness in any form whatever. This is precisely what gives to feeling its genuine biological significance, and makes its emergence a mark of a real step forward in general development. But I shall return to this point later, and meanwhile, in order that the relation of conflict to the occurrence of affect should be made clear, it is necessary to ask exactly how our normal responses *may* be held up or obstructed.

There are three conditions of overwhelming importance. First, one tendency to respond may be hindered because it is directly blocked by another, or by others. Second, a tendency to respond may be hindered because the cognitive material with which it has to work is imperfectly discriminated, blurred, ill-defined and relatively formless. Third, there is the extremely interesting case in which a response is not treated as complete until it is supplemented by other responses related to it through the operation of the same general interest which it itself arises to satisfy. This third case shows at once that feeling appears not only in consequence of what is happening contemporaneously with its appearance but also, in some instances, in consequence of what is going to happen in an as yet unrealised, or vaguely realised, future.

§ 2. AFFECT ARISING FROM THE CONFLICT OF TENDENCIES.

Of these three cases the first only is clearly and unmistakeably one of conflict—where one tendency wars against another one. Even then the clash need not produce affective experience unless it takes place above a certain level of development, and unless the battle is drawn out, the opponents being not too unequally matched. How long, in any given case, the struggle must continue for affect to appear is a matter for empirical determination. Experiments might decide, but naturally the question is a delicate one. A further generalisation may be made at this point: all the affects which grow out of this direct conflict are initially depressing, and often have a very marked unpleasing character. But this is a generalisation which is subject to considerable modification.

A typical mood-affect coming under this head is that belonging to disappointment; a typical emotion-affect is that belonging to fear, or anger. Disappointment arises where any tendency is obstructed by circumstances over which the subject has no control, but the tendency continues in spite of the check, and yet no violent and dramatic affect ensues. Fear and anger are special, defined cases of the same general conditions, and might perhaps be expressed in terms of the tendencies checked and the mode of their checking. It might appear as if disappointment does not involve a battle of tendencies, but only the removal of objective conditions for the satisfaction of a given tendency. But this is obviously an incomplete analysis. The removal of objective conditions never *merely* leaves the tendency vainly striving, but demands new modes of adjustment on the part of the subject. It is the resulting clash which gives rise to the affect.

Now in this case all the perceptual, imaginal or ideal material involved may be as clear as daylight. We may know perfectly well what we desired to do before the tendency was checked; we may perfectly discriminate the checking agency, we may be able to define exactly what new adjustments are needed and to what they are likely to lead. So far as the *material* factors go it seems certain that they may have no immediate reaction whatsoever from 'the unconscious,' and yet the affective accompaniment may be present and even marked.

§ 3. AFFECT ARISING FROM IMPERFECT DISCRIMINATION.

The second general condition for the arousal of affect was that of the obstruction of a tendency to respond, because the cognitive material involved is blurred, ill-defined, and relatively formless. The readiness with which affect appears to arise in connection with imperfect discrimination has often been noticed under experimental conditions[1]. But for the present purpose it is perhaps more interesting to consider almost any ordinary mood of depression. If a man who is depressed is asked why he is depressed he can usually give some reason. He will refer, often, to some special incident which may be perfectly clearly envisaged. But if he is then asked "is that all?" he generally confesses that it is not, saying, however, that whatever else is involved is something that cannot be clearly formulated.

Such inability to formulate itself calls for study. In many cases it appears to be due directly to some tendency which is frequently called 'repression.' Repression, however, as far as I can see, never arises except where some cognitive material gives rise simultaneously to two, or more, opposed tendencies. Then, if one of these is the stronger, the meaning which belongs to the material through its relation to this tendency remains uppermost, while that which belongs to the material through its relation to the other tendency is repressed. In so far as meaning is thus held under, or repressed, if it recurs at all it does so only in a relatively fragmentary or ill-defined fashion. Perhaps it might be argued with success that cognitive materials *always* are blurred, indefinite, and incomplete, unless they are accompanied by the unfettered expression of some active reaction tendency with which they are closely related. However this may be, the affect which does notably accompany ill-defined material should be referred, not to the blurring of the cognitive material, but directly to the conflict of active tendencies itself.

---

[1] See, *e.g.* this *Journal*, XI, pp. 322–4, 330–1.

At this point it is interesting to consider whether imperfect discrimination of material is *invariably* accompanied by affect. The answer is that imperfect discrimination simply has no psychological significance whatever except in relation to the activity or tendency that is functioning. From one point of view, for example, my discrimination of the detail of a cinema film when I see it in the ordinary way in a theatre, is very imperfect. The film contains much detail which I do not notice, or see but vaguely. So long however as I see enough to enable me to follow the general thread of the story, all is well. There may be affect in this case also, of course, but that depends more upon the general nature of the story than upon any particular discrimination of detail. Let the same film be put before me in a psychological laboratory, however, and let my discrimination of material be as complete as, and no more complete than, before; and at once the whole process may become for me highly disappointing.

This kind of example at once throws a doubt upon any attempt to express the conditions of affect in terms of the objective arrangement of the material making up any given situation. "Whenever," we are tempted to say, "material is presented rapidly, or slowly; is extensive or contracted; is intense, or slight, or is objectively characterised in some other way so that it cannot be adequately discriminated, there is affect." This is the kind of formula used by Ward, for instance, in his *Psychological Principles*[1]. It is extremely unsatisfactory. All the objective characteristics have importance only in reference to tendencies to response on the part of the subject. A series of objects passing before attention at a given rate is now too fast, now too slow, and now 'just right.' A stimulus now just sufficiently intense is again too slight and yet again too intense. Moreover, even for purposes of the most abstract theorising there is no point whatever in speaking as if there were one given objective rate of serial presentation, or one given intensity, or one given area of field, which is accurately adapted to the activity of one given tendency. A rate is too fast or too slow because it hinders the contemporaneous exercise of two tendencies or groups of tendencies. And further, if a given rate, being what we call 'just right,' seems to set up an affect, this is because it stimulates the contemporaneous exercise and progressive satisfaction of at least two tendencies or groups of tendencies.

---

[1] Ch. x. I possess a copy of Ward's *Encyclopaedia Britannica* article with annotations in his own handwriting. One note reads as follows: "If feeling as pleasure-pain determines selection, and feeling is a passive state or 'affection,' are we not creatures of circumstance? If we are not, and action is determined by 'what interests us,' what is the precise connection between feeling and interest?"

This view fits the cinema illustration perfectly. In the laboratory there are all the perceptive tendencies directed upon the discrimination of the detail of the film. But there is also, dominating and directing these, the group of tendencies with which a subject meets the demands of psychological experiment. If a subject knows nothing about the nature of experiments with films in a psychological laboratory, the chances are that so long as he can follow the general trend of the incidents depicted he is content. But if he does know what is likely to happen, and is at the same time unable to discriminate all the detail, he is disappointed, because in all probability he will be asked for the details later and expects that he will be classed with the fools if he cannot repeat them.

In the theatre the group of perceptive tendencies is operative as before. But these are now dominated by the group of responses belonging to interest in the theme represented. So long as the general trend of the theme can be followed, which is just as long as the group of interest responses can function together with the perceptual activities without interference from any others, then the mere *amount* of detail discriminated is of subordinate importance.

All this might appear to amount to the very common statement that there is affective tone of a pleasurable kind if such and such a tendency is facilitated, and of a painful kind if such and such a tendency is inhibited. But it seems to me that this kind of formulation is objectionable on various grounds:

(1) I find it extremely hard to admit that mere facilitation of a single tendency ever genuinely does give rise to affect. In fact the more a tendency is isolated in its expression and at the same time facilitated the less does affect arise. The performance then drops to the habit level, and as unobstructed habit is relatively free from feeling. The very fact that those writers who desire to emphasise facilitation, find that they have to try to express the conditions of facilitation in terms of some

P16298

objective arrangement or character seems to indicate that in the cases in which affect does occur it accompanies, not merely growing ease in the performance of a single tendency or group of tendencies, but the synchronous exercise of more than one response or group of responses.

(2) The term inhibition seems to be used in many different senses in psychological writing. But the notion that is required for the cases of unpleasurable feeling-tone is rather that of obstruction of a tendency, than of its definite inhibition; or of alternation from one tendency to another, with neither reaching satisfaction.

(3) In no case do I believe it to be possible to formulate the conditions of the arousal of affect in terms of the expression of single or unit tendencies. The essential factor is always to be found in a relationship of tendencies. Thus it is literally true to say that all affect is derived affect.

According to this view then it is incorrect to say that affect is a way in which blurred, ill-discriminated, repressed and fragmentary material is *known*. Affect is not, in fact, the inadequate *knowing* of any sort of material whatsoever but is a matter of the balance of reaction tendencies which are called into operation when a given situation is presented. There are two great classes of cases. In the first tendencies conflict, and one obstructs another or others. The affect then has a displeasing character. In the other synchronous tendencies find progressive satisfaction, and here the affect has a pleasing character. The pleasing character of affect in the second case is, I think, directly connected with the fact that this is the characteristic case for the development by integration of genuinely new forms of reaction.

§ 4. DIFFUSION OF AFFECT.

The view of the determination of affect which I have just put forward raises in an acute form a question which is not as a rule given very much attention. Feeling is very often regarded as a kind of natural appendage of certain cognitive material. The material is presented, and its apprehension possesses a quality or accompaniment of affect; there is no more to be said. But I have asserted that in no case is feeling fastened to particular material. And this may seem a paradoxical or at least an unusual view. For in common talk we treat certain ideas or images as inherently pathetic, others as humorous, others as 'thrilling,' and so on; and certain sensory content is considered to be inherently pleasing or depressing. The pathetic or the humorous novelist has his stock-in-trade of images, ideas, words and phrases. The music-hall artist raises a laugh by painting his nose red, or by merely uttering certain catch words.

However, when we look into the matter this is just what would be expected from certain outstanding characteristics of feeling as an experience. For affect possesses a remarkable readiness of diffusion. Itself aroused by a shock or by a balance of tendencies it runs over the whole situation, including the cognitive details in which these tendencies are expressed with the very greatest ease.

In point of fact there are hardly any (if indeed there are any) reaction tendencies expressed on a psychological level which do not involve the operation of cognitive processes. Thus all occurrence of feeling is possible only where there is also some discrimination of the details of a situation. The feeling then runs over the cognitive detail which is discriminated, and is very apt to be attributed to this. We then say, mistakenly, that a given sensory, imaginal or ideal content is inherently pleasing or displeasing.

§ 5. TRANSFERENCE OF AFFECT.

There is another common characteristic of feeling which produces the same general result, which has been repeatedly shown to occur under experimental conditions, and which is a common fact of daily observation. This is a marked ease of transferability from one context to another. An excellent illustration appears in Thomas Hardy's novel entitled *Desperate Remedies*. Cytherea Grange has just seen her father fall from a high scaffold, and knows that he must be killed. "The next impression of which Cytherea had any consciousness was of being carried from a strange vehicle across the pavement to the steps of her own home by a brother and an older man. Recollection of what had passed evolved itself an instant later, and just as they entered the door...her eyes caught sight of the south-western sky and without heeding, saw white sunlight slanting in shaft-like lines from a rift in a slaty cloud. Emotions will attach themselves to scenes that are simultaneous—however foreign in essence those scenes may be—as chemical waters will crystalise in twigs and trees. Ever after that time any mental agony brought less vividly to Cytherea's mind the scene from the Town Hall windows than sunlight streaming in shaft-like lines."

It has often been shown that cognitive discrimination is much more constantly in change than is affective tone. The feeling which properly arises in one situation, and is then diffused over the cognitive content defining that situation, gets easily transferred to another which is in some way connected with the first. It is now attached, in a derivative manner, to images, ideas and trains of thought depicting or describing this second situation. In our normal reactions in these instances we can detect no clash and no balancing of tendencies. We then say that the feeling is the 'natural' addendum of the images, ideas or thoughts. In a strictly analytical sense we are right, but from a point of view of how the affect was actually determined we are definitely wrong.

§ 6. THE FUNCTIONS OF AFFECT.

We have now before us those general considerations which enable us to understand the functions of feeling in mental development. I will briefly recapitulate my position:

(*a*) All feelings of the displeasing or depressing class are due directly to a clash or conflict of reaction tendencies, one tendency or group holding up another tendency or group which nevertheless still strives to find expression.

(*b*) All feelings of the pleasing or exhilarating group are due directly to the synchronous expression of tendencies which, by coming and continuing in operation together, are integrated to produce some new mode of reaction.

(*c*) Feeling is very readily diffused over the whole of the situation in which it arises and thus comes to be attributed to some cognitive content which is discriminated.

(*d*) Feeling is very readily transferred from the situation in which it arises to other situations connected with the first in ways that can be specified.

Now by very general consent the range of adaptive behaviour is extremely wide, passing far beyond the limits within which we have any right to speak of feeling or any other form of conscious experience. Wherever there is adaptive behaviour, however, there is the possibility of conflict and of integration among reaction tendencies. But at the lowest level the adaptive reactions which conflict must simply fight it out among themselves, and those which are inherently strongest win. This means, however, that although the possible modes of reaction are themselves adaptive there is no genuinely adaptive regulation of the interrelations of the tendencies. Thus, in an environment which is in continual objective change, ill-adapted conduct must constantly ensue.

When feeling first arises it does so as a sign[1] to the organism concerned that there is conflict or that integration and the achievement of a new response is possible. Let us take the case of conflict, for it is always easier to discuss the unpleasing than the pleasing feelings. There are two ways in which affect of this character directly influences practical dealings with a situation. Unpleasing feelings are a 'sign' of the clash and hold up of tendencies. They thus accompany what we in general term states of indecision. But it appears to be the case that the continuance of unpleasing response possesses some inherent capacity for hastening its own end. The unpleasurable class of affects thus does not actually solve any difficulty whatever, but it does normally help to put an end to difficulties. It is as if somebody should declaim "something must be done," without however specifying in any way *what* should be done. The place and function of unpleasure in directing learning processes has been greatly exaggerated; nevertheless unpleasure does appear to stimulate the learning process. It is never the depressive feeling which indicates *how* the conflict is to be ended, but it *is* the depressive feeling which helps the conflict to cease.

---

[1] It will be evident to anyone who has read MacCurdy's very striking book called *The Psychology of Emotion* (London, 1925) that this view is substantially the same as the view of the determination of emotions which is there expressed. It was indeed a matter of considerable satisfaction to me to find that each of us, approaching the subject from different standpoints, had arrived at extremely similar conclusions. Each of us had in fact developed his position fairly fully before he knew the other.

Further, that readiness of diffusion and transference which characterise affect again make feeling an extraordinarily bad and inefficient aid to the solution of difficulties. For the affect which properly belongs to a conflict set up by one part of a situation very speedily spreads over the whole situation, and then, by acting as if the block should be attributed to the whole the organism may make precisely the wrong sort of response to put an end to its troubles. Or, again, affect which, in point of experience, is the same may appear to belong to situations which are, as a matter of objective fact, exceedingly different. Thus an organism mistakenly strives to put an end to a conflict in a manner which, perfectly well adapted to one situation, is extremely ill adapted to another. So far as the functions of affect go when an organism does that it is reacting in a perfectly proper manner, because within the affect itself there is no basis whatsoever for distinguishing the two situations.

And so, although feeling arises as a first tentative step towards supplanting a mere blind struggle among adaptive tendencies, it achieves but a small step forward, and is a bad guide in practice.

§ 7. FEELING AND IMAGING.

How is this difficulty dealt with? It is dealt with by an attempt further to define the conflict of which the affect is a sign. There appears to be one way only by which this can be done. That way is by calling upon *cognitive* tendencies for help, and by attempting a more conscious discrimination of the situations which have aroused the antagonistic reaction tendencies. The most straightforward method of doing this is to effect as direct a reproduction as possible of those situations. Thus comes the image, and we can see at once a biological reason for two commonly noted facts: the very close connection between affect and image, and the tendency for images to arise when some normal reaction is hindered. The image method of solving a conflict really represents a new start in mental development. It is the beginning of the growing dominance of cognitive reactions which in the end produces the greatest achievements of intellectual control. In one respect it is undoubtedly a very great step forward. It revives a past situation, or it makes more clear a present one, and it does not merely emphasise the need for the conflict to cease. But in another respect the image method is even more inefficient than the affective mode. The affect of the unpleasurable class seems normally to carry with it, as I have said, a tendency to hasten its own end. The image does nothing of the kind. Hence though the image may help to show *how* a practical dilemma may be overcome it provides no sort of drive for making the necessary effort. Thus, although the image method is now acquired the affect method cannot be wholly dropped.

§ 8. FEELING, IMAGING AND THINKING.

Images however have their own peculiar drawbacks. Feeling fails, broadly speaking, owing to its fatal liability to lead to confusion of different situations; images may fail owing to their almost equally fatal liability to particularise situations. A simultaneous arousal of tendencies towards two or more antagonistic modes of response leads to check or conflict, and the question arises, "What shall I do?" Up comes an image. The image more or less defines a situation in the course of which I did act in a certain way in the past. So I may act in that particular manner now. But the present emergency does not really reproduce accurately anything belonging to the past, and the result may be an exceedingly bad solution of my difficulty. There are, no doubt, other reasons why images fail in this sort of emergency, but this one is enough for the present purpose.

Yet a new way must be found out of the same general situation of conflict. We find this when we think. Once more we rely upon cognitive tendencies as distinguished from affective experience. We carry further the processes of discrimination which begin in perceiving and issue in imaging. We find our solution by a careful analysis of the situation which provokes the conflict. The analysis proceeds by isolating and generalising the elements which gave rise to the check, and the solution secured is on the whole a more adequate one because it gets farther away from the accidents of a particular environment. As before, however, in acquiring a new mode we cannot afford to discard utterly the old ones. For, each being an attempt to deal with the same general problem in different ways, each has its own peculiar excellences as well as its own shortcomings. Hence thinking, imaging and feeling, all springing out of a conflict between reactive tendencies in behaviour, and the last two defining and determining the conflict, persist side by side. It is in fact as leading a principle of mental development as it is of biological evolution[1] that real advance is dependent upon the fact that the acquisition of new specialisations does not of necessity shut us out completely from the older and more primitive modes of response. But it is easy to see that thinking, being a further development of the cognitive reactions which are important in perceiving and imaging, is further removed, psychologically speaking, from feeling. Hence it is apt to be a relatively non-affective reaction, and the two may even appear antagonistic. However there are many instances in daily life in which, though thinking is dominant, affect is not only present, but marked.

§ 9. THE PLEASURABLE FEELINGS.

In the last two sections of this paper I have spoken more particularly of feeling, imaging and thinking as arising directly out of conflict. But what of the pleasurable class of affects? These are always more difficult to deal with. Perhaps, as unpleasure may be considered to possess some inherent capacity of hastening its own end, so pleasure may be considered to possess some inherent capacity of maintaining itself. But according to the general terms of the theory which I have accepted of the origin of feeling this obviously cannot mean simply that pleasure functions in maintaining a state unaltered. For it has been urged that the pleasurable feeling arises from progressive satisfaction, or is a conscious sign that the tendencies which are aroused are well balanced and are proceeding somewhere. *Different* tendencies are simultaneously operative as much in pleasurable as in unpleasurable feeling. Only in the first the difference does not hold up response, whereas in the second that, broadly, is what does occur. Just as unpleasure is a 'sign' that there is a conflict to be somehow resolved, so pleasure may be a 'sign' that a new reaction tendency involving the co-operation and integration of a number of distinguishable reaction tendencies is in process of acquisition. To maintain the pleasure is to carry further the process of acquiring new reactions. The new synthesis of response secured in this way does not need, it should be noticed, to issue in a satisfactory practical adaptation. Thus, in a learning process, pleasurable tone may accompany the progressive satisfaction of tendencies which do not, as a matter of actual fact, issue in what would be called a correct adaptation —say a solution of a maze problem. And once again, if this view is a true one, the function of feeling in leading to objectively successful response has often been greatly exaggerated, just as that of unpleasure in preventing unsuccessful response has been exaggerated.

---

[1] Cf. G. Elliot Smith, *Essays on the Evolution of Man*, Oxford, 1924.

Still practical needs compel the developing mental life to take steps to secure that the integration achieved is not merely pleasing but is also successful. And here, as before, the functions of imaging and of thinking come into play. But the images and the thoughts which are complementary to the pleasure class of affects are, on the whole, directed forward not backward. They are the definition of what is to be, and not the echo of what has been. Needless to say in mental life as we know it pleasurable affect and unpleasurable, forward imaging and thinking and backward, are intimately and indeed inextricably intermingled.

(*Manuscript received* 12 *February* 1924.)

A COMPARATIVE STUDY OF THE MATHEMATICAL ABILITY OF BOYS AND GIRLS IN SECONDARY SCHOOLS[1]

BY ANNIE E. CAMERON.

1. *Aim of the experiment* (p. 29).
2. *Subjects tested* (p. 29).
3. *Analysis of 'Mathematical Ability'* (p. 30).
4. *The tests:*
    - (a) *Justification for the use of original tests* (pp. 30, 31).
    - (b) *The subject matter of the tests* (pp. 32, 33).
    - (c) *The aim of each test* (pp. 33, 34).
    - (d) *The conditions under which the tests were applied* (pp. 34, 35).
5. *Manipulation of the tests:*
    - (a) *Method used for the comparison of boys and girls* (p. 35).
    - (b) *Elimination of irrelevancies* (pp. 35, 36).
6. *Results of the investigation:*
    - (a) *Comparison of groups of boys and girls* (pp. 36–42):
        - (*i*) *In separate schools;* (*ii*) *in co-educational schools.*
    - (b) *Correlation of the separate tests with the composite* (p. 42).
7. *The value of the experiment:*
    - (a) *Actual facts revealed* (pp. 42, 43).
    - (b) *Conclusions suggested by these facts* (pp. 43, 44).
    - (c) *Theories deduced* (pp. 45–49).

1.

THE aim of the experiment was to discover, by means of the application of mathematical tests, whether there exists any difference in mathematical ability between boys and girls in secondary schools, and, if so, to obtain some measurement of the amount and range of such difference and to estimate its significance.

2.

The enquiry was carried out, after preliminary testing, in February and March, 1923. The final tests were applied to 500 pupils in 14 secondary schools, of which 6 were girls' schools, 5 were boys' schools and 3 were co-educational schools.

---

[1] Read before the Education Section of the British Psychological Society, April, 1924.

3.

Measurements that have been made of mathematical ability in school-children have often treated the sections of mathematics—arithmetic, algebra and geometry—as wholes. But it is now becoming generally acknowledged that so-called 'mathematical ability' is a composite of various factors which are brought into play in different degrees and combinations in the several branches of mathematics. Some recent experimenters have recognised the composite nature of mathematical ability[1], though very little experimental work has been attempted with a view to investigating the special factors that enter into secondary school mathematics.

The tests used in this experiment were constructed so as to include what appeared to the experimenter to be the main factors of mathematical ability. These powers may be indicated as lying along the following lines: Power of analysis of a given situation and re-combination of its elements in a new way, power of comparison and classification of numerical and spatial data and of inference from them, ability to apply general principles and to manipulate abstract quantities, power of concrete imagination[2], and facility in mechanical computation. As far as possible each test was arranged to give prominence to one of these factors only, although it was not found practicable entirely to preclude the entrance of other factors.

4.

(*a*) The tests were based somewhat on the general ideas underlying those of Dr Agnes Rogers[3]; yet for several reasons they vary considerably from these, both in form and subject-matter. In the first place, the majority of her subjects had had only five months' formal mathematical training, while most of the subjects of this experiment had had 2½ years. The tests on 'Series' were, therefore, unsuitable, as the problems might not be novel in this case. Moreover, since the present tests were not applied by the experimenter, but by the ordinary secondary school teachers, and since the time granted for testing was limited, the tests had to be arranged with a minimum number of directions.

The problem paper that was constructed in arithmetic was made more difficult and longer, as it was considered that a fair estimation of the ability to solve problems could not be obtained from Dr Rogers' test, which was too short, too easy and not sufficiently varied for pupils in their third year at a secondary school. It seems strange that in testing mathematical ability, Dr Rogers included only one type of arithmetical test.

---

[1] *E.g.* W. Brown, C. Burt, D. Collar, H. O. Rugg, W. H. Winch.

[2] Defined below.

[3] *Experimental Tests of Mathematical Ability and their Prognostic Value*, 1918.

A test on the manipulation of algebraical symbols was substituted for her test on matching equations and problems. It is apparent that the novelty of the style of her paper introduced a new element, so that the test not only required (as she said) "ability to translate verbal statements into algebraic symbolism," but demanded also the adaptation of the subject to a novel situation. Dr Rogers states that the purpose of this test is to test 'mathematical intelligence' rather than "efficiency in a habitual method of working." But is she not attempting to test two opposing factors at the same time, since the construction of equations tends to become 'habitual' as the power of translation into algebraic symbolism is developed? Surely 'mathematical intelligence,' in the sense of the power to cope with a novel mathematical situation, requires a separate test. Again, when this test as constructed by Dr Rogers was applied by the writer to a group of students, it took some of them from twenty to thirty seconds to adapt themselves to the novel type of question. The test, in fact, mainly measured power of adaptation to novel circumstances. A similar factor also occurs in the test on matching *n*th terms and series.

In the Geometry Deductions test, the fact that the pupils were not allowed to use in their proofs facts drawn simply from their own experience, but were given a body of facts on which to base their reasoning, served to complicate and increase the difficulty of the test, and to make it somewhat artificial.

The Geometry Definitions test would be of use in the form set only where the subjects had not previously defined the various figures. It could therefore be applied only to pupils at an early stage of geometry.

For these reasons it was considered desirable to construct a fresh set of tests. The Algebraical Computation test, the Superposition test and the Drawing of Sections test were based on the similar ones given by Dr Rogers. The subject-matter of the first of these was entirely altered, and the second and third were considerably altered in form in order to shorten the directions necessary for carrying them out.

(*b*) The subject-matter of the tests was as follows:

ARITHMETIC.

Test I. Calculation. Time 7 minutes.

16 questions of the types:

(1) 42×99. (2) $49{,}005 \div 121$.

(3) $24 \times 13 = 9 \times 13 - 3 \times 13$. (4) $8\frac{1}{2}d. \times 480$, etc.

Test VIII. Solution of problems. Time 20 minutes.

17 arithmetical problems.

Test IX. Construction of problems. Time 15 minutes.

7 questions, such as:

(1) Construct a question in *Simple Interest,* for which the following working will be suitable:

$$\text{Simple Interest} = \frac{£5 \times 7 \times 155}{100} = £54{\cdot}25.$$

Answer: £54. 5*s.* 0*d.*

(2) Construct two problems on *Profit and Loss,* each involving a *percentage,* for which the following solutions will be suitable:

$$(a)\ \text{Selling Price} = \frac{£95 \times 72}{100} = £68{\cdot}4.$$

Answer: £68. 8*s.* 0*d.*

$$(b)\ \text{Cost Price} = \frac{£100 \times 52}{104} = £50.$$

Answer: £50.

ALGEBRA.

Test II. Manipulation of mechanical processes. Time 5 minutes.

Test III. Use of general symbols. Time 5 minutes.

14 questions, such as:

(1) 50 articles cost 4*s.* How many shillings do n articles cost?

(2) A boy bought x marbles and y pencils for 1*s.* He lost 3 of the marbles and sold the rest at 3 a penny. He sold the pencils at 2*d.* each. How much money did he gain?

(3) A is 3 times as old as B. If B's age is now x years, how old will A be n years hence?

GEOMETRY.

Test IV. Superposition. Time 2 minutes for reading instructions and 2 minutes for marking figures.

Eleven rectilineal figures of different shapes were given, named A, B, C, etc., 19 unnamed figures were also given. The pupil was instructed to imagine the former figures cut out so that they could be moved round into any position. He was to find any figures in the second set which he thought Figure A would fit *without being turned over*, and write A in them, and so on for the other letters. If he found any figures which he thought no named figures would fit *without being turned over,* he was to write X in them.

Test V. Construction. Time 8 minutes. The directions were:

"You are given certain figures and required to construct figures similar to the ones given. The foundation lines are given you on which to build up your constructions. Since these are *not of the same length* as in the given figure, your drawing will *not be of the same size* as the given figure."

"No protractors are to be used. No construction lines are to be drawn on the given figures."

Test VI. Deduction. Time 6 minutes.

The pupil was required to supply the omitted steps in reasoning in 4 geometrical exercises. The omissions were indicated by dotted lines.

Test VII. Drawing of sections. Time 6 minutes.

Eight questions. The pupil was required to make sketches of surfaces obtained by cutting geometrical solids in various ways.

The general instructions to the pupils were:

I. Tests to be worked in pencil.
II. In each test *attempt* the questions in order. If you find any question you cannot do, leave it and go on to the next.
III. Work as *accurately* and as *quickly* as you can, but do not mind if you do not finish.
IV. Do not go over the working of any question a second time.
V. *Read the instructions on the top of the test* before you begin working.
VI. *No rough working* is allowed.

(*c*) Tests I and V were numerical and spatial tests respectively. The aim of these was to discover how rapidly the subject could analyse a given situation into its elements. The subject was required to give evidence of this power by recombining the given elements in Test V and by combining the elements in a new way in Test I. Tests I and V were both, then, tests of analytical ability, but Test I required a further power of synthesis. In Test I the questions were so planned that it would be a difficult task to obtain the answers without some resort to analytic methods, since no rough working was permitted. Even if some answers were managed by working out the questions without 'short cuts,' very little would be accomplished in the given time, and there would be great liability to error.

The aim of Test III was to estimate the subject's power of expressing in general form simple relations between quantities. The test involved scarcely any calculation. The element of abstraction was given prominence and the problematic element reduced to a minimum. The ability needed to solve a problem in algebra appears to involve a factor similar to that involved in solving an arithmetical problem, together with ability to manipulate abstract quantities. It was the latter ability that was measured in this test, the former being measured in the Arithmetical Problems test.

Tests IV and VII dealt with spatial relations. The former tested the power of observation and comparison of form by means of imagined superposition. The latter tested the power of realisation of more complex spatial relations, and involved a higher degree of visual imagery.

Test VI assumed the knowledge of elementary theorems only. It was presented in a somewhat unusual form, generally limiting the subject to a particular method of proof, in order to measure his capacity to follow a line of deductive reasoning intelligently as well as to continue the argument.

The problems in Test VIII were arranged so that the numbers could easily be carried mentally. It was thought that grasp of the whole situation could be better tested if written work were not allowed, and that loss of time in purposeless playing with figures would be avoided. In the revision of this test after the preliminary application, the questions were rearranged in order of difficulty, as determined by the first set of results, the problem which had been solved by the greatest number being placed first, and so on.

Test IX tested the power of expressing concretely an abstract situation and involved the recognition of combinations of figures as representing certain situations.

(*d*) The tests were combined into two sets, each set covering approximately one school period. Tests I–VII were taken together at the first sitting, and Tests VIII and IX at the second[1]. In every case there was an interval between the sittings, generally of about one to two days. In all cases except one, Tests I–VII were applied in morning periods. In five schools, Tests VIII and IX were applied in the early afternoon, and in the others in the morning. The tests were generally carried out in the mathematics lesson periods by the ordinary mathematical teachers. This was considered advisable in order to keep the element of novelty in the background and to reduce excitement to a minimum. Detailed directions for the application of the tests were sent to the schools. These appear to have been exactly carried out, except in the one case quoted above.

The forms tested were in their eighth term of algebra and as far as possible in their eighth term of theoretical geometry. The enquiry was limited to pupils at about the same stage of mathematics, so as to ensure that the groundwork should approximate in the different schools. It was found that the average age of the boys taking the tests tended in the separate schools to be somewhat lower than that of the girls. This was due to the fact that, in the boys' schools tested, formal mathematics was often started some months earlier than in the girls' schools. In the co-educational schools, the average ages of the boys and girls were approximately equal.

---

[1] Except in the case of Group F 1, which took Tests I–VII in two separate sittings and Tests VIII and IX in separate sittings.

5.

(*a*) To effect the comparison of boys and girls, the percentage of girls who exceeded the median score of the boys was obtained in each case.

In this study the median has only been used as a basis of comparison between boys and girls, and in all cases where the median falls within a group, its value is stated as the value of the particular group in which it falls[1]. If it falls between two groups, its value is reckoned as being mid-way between the values of the two groups. In the cases where the median of the boys falls within a group, the number of girls who exceed the median of the boys is estimated in the following way:

Let r be the value of the group, and let there be m boys with score r. Suppose that the position of the median falls between two cases in this group such that there are n boys in the group above it. The number of girls above the median of the boys is reckoned as the number who scored more marks than r, together with a number represented by $\frac{n}{m} \times p$, where p is the number of girls who actually obtained score r.

When, theoretically, the position of the median falls exactly on one of the numbers of the group, which happens when the total number of boys in the list is odd, if there are n boys above the median in the group in which the median falls, there are reckoned to be $\frac{2n+1}{2}$ boys above the median in this group. The number of girls above the median of the boys is the number who actually scored more marks than r, together with a number represented by $\frac{2n+1}{2m} \times p$.

(*b*) A general survey of the results of the tests brought to light the fact of the unevenness of achievement of the various schools. This made the comparison of the results a more difficult task, and may have been due partly to the fact that the schools were selected from different parts of England and Wales. It is to be expected, however, that any conclusions as to general tendencies will be more reliable if drawn from data obtained from a wide area.

---

[1] The theoretical numerical value of the median is not required.

In comparing the groups, it was necessary to make allowance for four irrelevant conditions:

1. *Difference of locality.* This difficulty was anticipated in selecting the schools, and where it was possible one girls' and one boys' school were chosen in each district for comparison.

2. *Difference of social status.* This probably was too slight to affect the results considerably. The type of school was the same throughout, and the pupils tested were nearly all ex-public elementary scholars[1].

3. *Difference of general intelligence.* It was found impracticable, often because of exigencies of examinations, to obtain all the pupils at the assigned stage in all the schools. In three cases only one[2] of two parallel forms was tested.

4. *Difference of age.* The ages were limited to a range of 13–$16\frac{5}{6}$ years. This necessitated the scores of 9 girls and 5 boys over $16\frac{5}{6}$ years of age and the scores of 1 girl and 3 boys under 13 years of age being omitted from the results.

6.

The mean scores, the standard deviation from the mean and the probable error of the mean were found for each group in each of the tests. The mean was almost in every case considerably greater than six times the probable error. The standard deviations were found to be greatest on the whole in Test VII and least in Test II. The degree of scatter for the separate tests varied considerably from school to school.

(*a*) (i) For purposes of comparison the separate schools were grouped in pairs, each pair containing a girls' and a boys' school in the same district or a similar type of district.

Table I gives for each pair of groups the percentage of girls who exceeded the median of the boys in each test. In the Table the averages of the percentages are shown, with and without Groups D 1 and D 2[3], and in the case of Test VII, also with and without E 1 and E 2 *a*[4].

---

[1] Group F 1 was an exception. About half of Groups D 1 and D 2 were from elementary schools.

[2] The 'a' form.

[3] In the case of Group D 2, only the boys (except two) who were under 15 years of age were tested. In order to equalise the conditions between the two Groups D 1 and D 2 in this respect, all the girls of Group D 1 who were 15 years of age and over (except two) have been excluded from the results. How far this selection has affected the comparison between the two schools cannot be judged.

[4] E 1 was the only group which had done some work in 'sections.' It seems to be this previous practice that has raised the central tendency for this group to a comparatively high level.

TABLE I. *Comparison of Girls with Boys. Separate Schools.*

	Test I		Test II		Test III		Test IV		Test V		Test VI		Test VII		Test VIII		Test IX	
	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.
Group B 1 with Groups A 2 *a* and A 2 *b* ...	4·5	28·0	41·5	40·0	8	64·0	4	50·0	8·5	52·0	8	42·7	3	22·9	13	24·0	6	55·2
Group C 1 *a* with Group C 2	3	33·0	39	8·0	6·5	32·0	6	26·0	8	66·7	4	78·6	3	49·7	12	28·8	6	51·5
Group D 1 with Group D 2	3	33·3	38·5	16·7	6·5	33·3	7	7·3	11·5	16·7	6	41·7	3	30·0	11·5	0	7	15·0
Group E 1 with Group E 2 *a*	5	45·6	36·5	60·0	12	45·6	5	24·1	12	33·6	7·5	63·3	4	82·0	14	65·6	9	40·0
Group G 1 *a* with Group E 2 *a*	5	34·7	36·5	72·0	12	36·0	5	-37·0	12	23·3	7·5	76·0	4	10·4	14	43·1	9	27·1
Average percentage ...	—	34·9	—	39·3	—	42·2	–	28·9	—	38·5	—	59·3	–	39·0	—	32·3	–	37·8
Average percentage omitting Group E 1 with Group E 2 *a*	—	—	—	—	—	—	–	—	—	—	—	—	–	28·3	—	—	–	—
Average percentage omitting Group D 1 with Group D 2	—	35·3	—	4·5	—	44·4	—	34·3	—	43·9	—	63·7	–	41·3	—	40·4	–	43·5
Average percentage omitting D 1 with D 2 and E 1 with E 2 *a* ...	—	—	—	—	—	—	–	—	—	—	—	—	–	27·7	—	—	–	—

M. = Score of the group in which the median of the boys falls.
P. = Percentage of girls exceeding the median of the boys.

Groups from corresponding schools or the same school are denoted by the same capital letter.
Girls' groups are denoted by the figure 1 and boys' groups by the figure 2 placed after the capital letter.
Groups marked *a* are slightly above the average in general attainment.
Groups marked *b* are slightly below the average.

Examination of Table I will show that only in the case of the pair of Groups D 1 and D 2 are the boys decidedly superior to the girls, and even this statement must be qualified in the case of Test VI, where the results are approximately equal. For, according to Starch[1], "any differences lying between 40 per cent. and 60 per cent. of the number of either sex reaching or exceeding the median of the other" are practically negligible. The discrepancies of any significance between the boys' and the girls' achievements, seem to lie in Tests I, IV, VI, VII and VIII.

Test VII. If Group E 1 is omitted, this test shows the greatest discrepancy between the girls and the boys, only 28 per cent. of the girls, on an average, exceeding the median of the boys.

Test I. In Test I the results show one group of girls equal to the boys, and four groups below; the average of the percentages of girls who exceed the medians of the boys being about 35.

Test IV. In this test also the results for the separate schools show one group of girls equal to the boys and four groups below, the average of the percentages of girls who exceed the medians of the boys being about 29–34.

The arithmetic means of the scores obtained by the groups of boys range from 4·5 to 6·9, with an average of 5·8, and those of the groups of girls from 4·0 to 4·8, with an average of 4·4. It may be noted that while the averages for the girls are all between 4 and 5, those for the boys are all (except one) over 5. So that in Tests I and IV there is a difference in favour of the boys, and this difference seems to be more marked in Test IV.

Test VI. In this test the girls seem to have, on the whole, a distinct advantage. It is the only one in which no pair of groups shows a decided difference in favour of the boys, and three pairs of groups show a decided difference in favour of the girls. Moreover, the results are more consistent among themselves than in the other tests (excepting perhaps Tests I and III).

There is a larger range in the case of the boys' means (4·2 to 9·9) than in the case of the girls' (5·8 to 9·3), but the average of the girls' means works out at 7·4 as against 6·8 for the boys.

Test VIII. The girls' Group E 1 has done exceptionally well on this test and has scored higher than the corresponding boys' school, in spite of the fact that the boys' result is well above the average and the fact, also, that the girls' group was an average group while the boys' was slightly above the average. Apart from this group, Table I shows the girls inferior to the boys in three cases and equal to them in one case. In this problem paper we may therefore infer that there is a tendency for the boys to be superior.

---

[1] *Educational Psychology*, 1921.

TABLE II. *Averages of the Arithmetic Means of the Scores of the Groups selected for comparison.*

	Age	Test I	Test II	Test III	Test IV	Test V	Test VI	Test VII	Test VIII	Test IX
Groups B1, C1*a*, D1, E1, G1*a* ...	15·3	3·5	36·4	8·1	4·4	9·4	7·4	3·2	10·9	6·9
Groups A2*a*, A2*b*, C2, D2, E2*a*...	14·6	4·3	40·2	8·9	5·8	8·9	6·8	3·5	13·8	7·0
Groups H1, J1, K1	14·3	4·1	31·9	9·0	5·5	9·0	6·4	3·2	10·9	6·7
Groups H2, J2, K2	14·3	4·7	31·9	10·4	5·9	9·7	7·0	4·6	14·9	8·0

In the remaining tests the indication is of a very fair equality between girls and boys, the average percentages ranging from about 43 to 45 (Table I). In Test IX the average of the means for the boys' and girls' groups is about the same (Table II); in Tests II and III it is slightly in favour of the boys, and in Test V it is slightly in favour of the girls.

(ii) In none of the three co-educational schools do the boys show a decided superiority over the girls in all the tests (see Table III). In Test VII the superiority of the boys is still shown, the results from the co-educational schools comparing very closely with those from the separate schools (roughly 20, 30 and 45 per cent., as against 20, 30 and 50 per cent.), omitting the girls' group that did exceptionally well (Group E 1) and the one that did exceptionally badly (Group D 1).

The other two tests that in the separate schools showed a superiority in the case of the boys were Tests I and IV, this superiority being rather more marked in Test IV than in Test I. It is worthy of note that in all three co-educational schools tested there appeared to be no significant sex-difference in the result of Test I, the percentage of girls exceeding the median of the boys being in each case between 40 and 50, while in Test IV, whereas only one of the five pairs of groups from the separate schools showed the percentage of girls exceeding the median of the boys to be over 40 (Table I), two out of the three co-educational schools showed the percentage to be over 40, one of these percentages being decidedly over 60, which exceeded any percentage shown in the separate schools. In Test IV, then, the girls and boys from the co-educational schools are practically equal. This acquired equality is due to the higher level attained by the girls from the co-educational schools, both in Test I and in Test IV, as compared with that of the girls from the separate schools. The difference is greater in Test IV, where the average of the

TABLE III. *Comparison of Girls with Boys. Co-educational Schools.*

	Test I		Test II		Test III		Test IV		Test V		Test VI		Test VII		Test VIII		Test IX	
	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.	M.	P.
Group H 1 *a*+H 1 *b* with Group H 2 *a*+H 2 *b* ...	4	46·1	38	63·2	8	58·8	5	65·8	10·5	21·1	9	52·6	5	30·7	15	25·0	8	29·5
Group J 1 *a*+J 1 *b* with Group J 2 *a*+J 2 *b* ...	4	41·1	30·5	50·0	12·5	14·3	5	41.5	12	36·0	8	18·8	3	43·8	16	19·6	11	23·8
Group K 1 *a*+K 1 *b* with Group K 2 *a*+K 2 *b* ...	5	47·6	24·5	58·6	10	49·7	6	37·1	9	49·1	3·5	62·1	5	22·4	13	32.8	5	40·7
Average percentage... ...	–	44·9	—	57·3	—	[40·9]	–	48·1	—	35·4	—	[44·5]	–	32·3	—	25·8	—	31·3

M. = Score of the group in which the median of the boys falls.

P. = Percentage of girls exceeding the median of the boys.

means has increased from 4·4 to 5·5. The means of the scores of the groups of girls from the separate schools for this test are all between 4 and 5, while in the co-educational schools they are all between 5 and 6. So that in both these tests the superiority of the boys as shown in the separate schools seems to disappear in the case of the co-educational schools.

Test VI shows such discrepancy between Groups J 1, J 2 and the groups from the other two co-educational schools that it would be unsafe to make any general statement with regard to this test. This much, however, may be said, that, considering all the girls' results of Test VI in general, it is probable that the girls of Group J 1 may be rather below the general standard in deduction work, as this low percentage seems to be an exceptional one. Judging from the other two results, the girls seem to maintain their slight superiority in this test, though it may be noted that while the boys from the separate schools and from the co-educational schools show about equal average results, the girls from the co-educational schools have not done so well on this test as the girls from the separate schools (see Table II).

In Test VIII all the three percentages show a distinct superiority on the part of the boys. This is the test that manifested the greatest sex-difference in the co-educational schools. The sex-difference is greater here than in the separate schools, owing to the fact that the boys, as a whole, have done better than the boys from the separate schools.

In Test IX the boys show a superiority in two out of the three pairs of groups (see Table III). In the third pair of groups (K 1, K 2) the averages of the boys' and girls' scores are exactly equal. The average of the means for the boys from the co-educational schools is higher than for those from the separate schools (see Table II), which has brought about an increase in the sex-difference.

In Tests II and III the equality between the boys and the girls seems to be maintained on the whole, though in Test III, as in Test VI, the pair of groups, J 1, J 2, shows a great discrepancy as compared with the other two pairs of groups. Test II is the only one in which both the boys' and the girls' groups from the separate schools have achieved, on an average, better results than the groups from the co-educational schools (see Table II), there being slightly more difference in the case of the boys, with the result that the girls from the co-educational schools compare rather more favourably with the boys.

Test V shows the girls inferior in two cases out of three.

In every test, except Test II, the boys in the co-educational schools have done better on an average than the boys from the separate schools.

The girls have done better in Tests I, III and IV, equally in Tests VII and VIII, and worse in Tests II, V, VI and IX.

Comparison was also made between groups selected on an age basis. The results of this comparison agreed fairly closely with the others.

(*b*) A composite for the sum-total of the mathematical abilities tested was also obtained, and the separate tests were correlated with this (see Table IV).

The group selected for this purpose was the group of boys from the co-educational schools who were between the ages of 14 and 15, as this was one of the most homogeneous groups and represented the average boy. The number in the group was 52. The composite deviations were obtained in the following way:

The individual deviations of the scores from their mean were first calculated and the standard deviation obtained for each test. Each individual's achievements in the different tests were then made comparable by dividing the individual deviation in each test by the standard deviation. One-ninth of the sum of these deviations for each individual in all the tests was reckoned as the new deviation in the composite.

The co-efficients of correlation were then worked out by the formula $r = \frac{\Sigma (uv)}{n}$, where r = the coefficient of correlation, u = the individual deviation in one test divided by the standard deviation, v = the final individual deviation in the composite, obtained by the above method, n = number of boys.

TABLE IV. *Co-efficients of Correlation of the separate Tests with the Composite.*

Test	r	PE
I	·47	·08
III	·45	·08
IX	·44	·08
V	·42	·08
VIII	·42	·08
II	·33	·09
VII	·31	·09
IV	·26	·09
VI	·22	·09

r = Co-efficient of correlation.
PE = Probable error.

7.

(*a*) The outstanding fact revealed by the experiment is that in the results of these tests considered as a whole neither sex is distinctly superior to the other. It must, however, be remarked that in the cases where the boys and the girls have been stated to be approximately equal, the percentage of the girls exceeding the median of the boys tends to be between 40 and 50, and on all the tests together works out at about 40. Thus there is a slight general tendency to superiority on the part of the boys. The difference in favour of the girls in Test VI, though only slightly over 10 per cent., gains greater significance from this fact.

The most marked superiority of the boys is in Tests VII and VIII, where a sex-difference is indicated both in the separate and in the co-educational schools.

Tests I and IV show on the whole a difference in favour of the boys in the separate schools, but no significant sex-difference in the co-educational schools. In both cases the girls in the co-educational schools have done better on the whole than the girls in the separate schools, the difference being more marked in Test IV.

Test VI shows a difference in the separate schools in favour of the girls, but the results in the three co-educational schools are too uneven to permit of any deductions being made from them.

In the other tests the achievements of the boys and girls seem to show on the whole no significant sex-difference.

(*b*) It is recognised that from such a small number of groups no definite conclusions as to the comparative mathematical abilities of boys and girls in general can be drawn. Yet, in respect of the particular groups tested in this experiment certain tendencies seem to be indicated.

1. There appears to be no significant sex-difference in mathematical ability taken as a whole, when the separate mathematical factors are given equal weight.

2. It seems, from consideration of the results of Tests IV and VII, that in the separate boys' and girls' groups, ability in spatial observation and comparison in two dimensions, and power of visual imagery of forms of three dimensions, is somewhat stronger in the boys than in the girls. When the boys and girls are taught together, the superiority of the boys in the special form of two-dimensional comparison contained in Test IV tends to disappear, owing to the higher standard reached by the girls, whereas the superiority of the boys in the imagery of the more complex forms is maintained.

3. Judging from the results of Test VI, the girls seem to possess a superior power of deductive reasoning from spatial data.

4. Test V, as has been stated, was formed with the purpose of estimating the comparative analytic power of boys and girls. In the mental ability involved it has much in common with the mechanical

problems set by Dr H. B. Thompson[1]. For the parts of the given figure had to be realised in their essential properties and order of construction, so that a facsimile of it might be produced.

Dr Thompson suggests that the superiority which she found in the mechanical ability of boys and men comes from the fact that they are more accustomed to dealing practically with mechanical objects, and that the essential factor involved in their superiority is greater mechanical interest on their part. Burt and Moore[2] do not agree that this is the only factor at work, though they likewise found the boys superior in a mechanical puzzle test.

The results of Test V seem to confirm Dr Thompson's opinion. For if this test measures the same ability of analysis and reconstruction as the mechanical puzzles tests, the conclusion would be that the excellence of the boys depends upon the class of object on which the ability works. If it is a case of dealing with a concrete and mechanical situation, the boys are superior to the girls, but if the situation is as familiar to the girls as it is to the boys, the superiority of the boys seems to be greatly diminished.

5. In algebra, both in mechanical calculation and in intelligent manipulation of abstract quantities, the boys' and girls' achievements were approximately equal. Therefore, if there is a sex-difference in algebraical ability, it probably does not lie along either of these lines.

6. Test I, like Test V., was a test of analytical ability. If framed to test simply the same power as Test V it should have produced a similar result. But although the material of Test I was as familiar to the girls as to the boys, the test showed a significant difference in favour of the boys, when the results from separate schools were considered. The reason lies in the fact that it involves a further type of ability than that of analysis. The suggestion has already been made that this further power is that of synthesis. It must then be this element which has produced the sex-difference.

7. In the ability to solve arithmetical problems, the boys showed themselves superior to the girls, whether taught separately or with the girls.

8. Test IX required ability to express concretely an abstract situation, which is a special type of constructive ability often needed in mathematics. A difference in favour of the boys is indicated in the co-educational schools, but not clearly marked in the separate schools.

---

[1] *The Mental Traits of Sex*, 1903.

[2] "Mental Differences between the Sexes," *Journal of Experimental Pedagogy*, June and December, 1912.

(*c*) So far as it has been possible to obtain some measurement of the elements of mathematical ability by means of this experiment, the results seem to suggest that some of these elements are possessed by both sexes in equivalent amounts; some are stronger in boys in general, or under particular conditions; one may be stronger in girls.

It is proposed to deal first with the tests on superposition and arithmetical calculation (IV and I), in which the girls seemed inferior to the boys only when they were taught in separate schools. The former of these was a test on spatial comparison. In view of the fact that the power to realise spatial relations and to classify spatial data bulks largely in geometry, it would be well to enquire if there is any element involved in this power which would seem to belong to the male mind rather than to the female. The former seems to be more prone to see things in their entirety, while the latter appears to have a tendency to concentrate on details. Since two minutes only were given for working this test, it was necessary for the subjects to take in at a glance the essential features of each figure and to see it as a whole. The pupil who would require to consider each side and angle separately in order to compare the figures would be at a great disadvantage. This was why the pupils were instructed not to make any measurements on the given figures. The purpose was to discover the rapidity with which the *whole* spatial situation could be grasped. It may be remarked here that the variability in the speed by which this test was done was very great. Some pupils attempted to mark only one or two figures, while others marked nearly all. Three boys scored 15 out of a possible 19, while many pupils failed to mark any figures correctly.

The difference between the boys and girls may be a matter of training. The man, from the nature of his work, has to deal with life in a more comprehensive way. He sees many more points of view and acquires the habit of looking all round a thing. The boy's mental outlook is directed along these lines.

The power of synthesis required for Test I is probably highly related to the ability of viewing objects and situations in a comprehensive way.

It is interesting to note that the girls in the co-educational schools seem to have developed these powers. If it could be proved that the latter can be developed in girls by means of suitable training and environment, it would establish the fact that the sex-difference which was found in the separate schools is merely a matter of training and environment. Now the difference in environment consists of the presence of the boys in the class and the sex of the teacher. Nearly all the girls from the co-educational schools had been taught mathematics by a master for at least two-thirds of the time they had been at the secondary school, and more than half of them for the whole time. They had all been taught with boys for several years. It may be that continual contact with the male point of view has broadened the outlook of these girls, developed in them a greater power of combination of ideas, and made their survey of life more comprehensive.

Beside these two tests, there is only one other in which the girls from the co-educational schools have shown themselves better than the girls from the separate schools. Thus the training in the co-educational schools does not seem to have produced in these girls improvement in mathematical ability as a whole, but only in one or two factors of mathematical ability[1].

With regard to Test VI it is possible that a girl's training, which develops in her a better grasp of details, might give her an advantage in this test. The paper was framed in such a way that realisation of the whole plan of the proof was not so much needed at the beginning, as ability to note the details of proof that were given, and to follow them out accordingly.

Moreover, the Report of the Consultative Committee on "Differentiation of the Curriculum for Boys and Girls respectively in Secondary Schools (1923)[2]" suggests that, in reasoning, the male and female minds as a rule travel by different routes. Girls "jump to a conclusion and reason back from it to test its accuracy," boys 'normally' reason "forward from the facts to the conclusion." If this is the case, the form of Test VI would give an advantage to the girls. For the paper consisted, as it were, of various stepping-stones. The girls would take each one of these and reason back from it to test its accuracy, and thus build up the whole proof. The boys, beginning to reason forward in their own way from the given facts to the conclusion at the end, would perhaps not find the stepping-stones quite fitting in with their line of argument and so prove them stumbling-blocks instead.

It has been shown that, in Test VII, previous training in 'sections' has greatly affected the result of one girls' group. It is a question whether previous training has affected also the boys' achievements in this test. A boy will have met with the idea of the cutting of sections in his practical woodwork, either at school or at home. He will have become acquainted in the course of his experience in this subject with some of their most common shapes and he will thus be able to image them more easily. One headmaster gave it as his opinion that a boy might be helped in this way, and the superiority of the boys on the test may be due partly or entirely to this cause.

---

[1] Facts that have come to light in the comparison of the age-groups seem to indicate that it is the most intelligent girls who profit by a co-educational training in mathematics.

[2] p. 95.

It is then unsafe to make any suggestions from Test VII with regard to the comparative ability of the boys and the girls, without further investigation. It would be necessary for this purpose to obtain groups of boys and girls who had had the same amount and the same kind of previous training in this ability.

The other test which showed a significant difference in favour of the boys was Test VIII. The suggestion was made that boys excel girls in arithmetical problem work. This raises the question: What is the fundamental factor in the solution of arithmetical problems? In this problem work it is necessary to be able to image the situation—to consider the symbols in connection with the objects which they denote. There is much purposeless juggling with figures in problem work because the pupil is endeavouring to get at the relation between the data without first having a mental picture of the problem. When the substance of the problem is imaged as a real thing, the parts are seen more clearly in their relation to one another and the solution is suggested. This power we may call that of 'concrete imagination[1].' The more highly developed this power is in the pupil, the more quickly will he be able to see his way to a solution.

Probably the sex-difference, if it exists, lies in the greater strength of this ability in the boy. That this sex-difference persisted in the co-educational groups tested, where the other sex-differences disappeared, suggests that the capacity may be inborn, and naturally stronger in the boy. Yet some girls seem to possess as great an ability for problem work as boys. The pupil who scored highest marks in this test was a girl, and a girl held the second place with two boys.

Whether this power is innate, or whether it is due to outlook, it will help to account for the alleged superiority of boys in school mathematical examinations, since it probably figures more largely there, where problem work of this kind is apt to predominate, than in these tests.

---

[1] Take the case of two boys of equal general intelligence, who are set to solve a problem of the type: "How long will it take a train a feet long, travelling at r miles per hour, to pass a telegraph post?" That boy will succeed better who not only grasps the whole problem, but also has a concrete image of the fundamentals in the situation, *i.e.* he images the position of the train before and after passing the post. This does not mean that he must have a 'visual image' of the actual train.

Thus general superiority of boys in mathematics may arise from the relative importance in mathematics of the ability or abilities in which boys usually surpass girls. In order to estimate this, the co-efficients of correlation of the separate tests with the composite were examined (see Table IV). The highest co-efficient was found to be ·47. The lowness of this indicates that no one of these tests by itself can be a test of mathematical ability in general, but that the latter is made up of a heterogeneous group of capacities, which separately have not a very close relationship to the whole.

The tests which correlated most highly with the composite were the tests on Arithmetical Calculation (I), Manipulation of Abstract Quantities (III), Construction of Arithmetical Problems (IX), Geometrical Constructions (V), and Solution of Arithmetical Problems (VIII). It seems then that the abilities involved in these tests have a higher relationship to mathematical ability as represented by the composite than those involved in the other four tests. Thus we should expect mathematical ability to be more dependent on these factors than on the others, that is, these components would hold a more important place in mathematical ability as a whole. The chief factors involved in these tests were the ability to analyse a situation into its elements and to re-combine them either in a similar way or in a new way (V and I), the ability to manipulate abstract quantities intelligently (III), to translate an abstract situation into a concrete one (IX), and the power that has been named 'concrete imagination' (VIII).

The four tests which bore the least relationship to the composite may be inferred not to hold an important place in mathematical ability. These were the tests on Algebraical Computation (II), the Drawing of Sections in Solid Geometry (VII), Superposition (IV), and Geometrical Deduction (VI). These tests seemed to involve rather isolated abilities. Mechanical computation does not appear to rank high in mathematical ability, nor does the drawing of sections, which involves visual imagery. That the superposition test does not hold a high place is not surprising, for, though it is the foundation of spatial comparison in general, in itself it has practically no common bond with the other tests, and for use in mathematics it must be combined with other capacities.

The low position of Test VI in the list leads to the assumption that the power of deductive reasoning is only a secondary element in mathematical ability. Here again the importance of this power depends on its combination with other elements. Solution of geometrical riders not only involves deductive ability, but the power of comprehensive outlook and grasp of the whole situation. By the particular form of the test this second element was eliminated.

Thus neither of the abilities tested in IV and VI will achieve much singly, but if combined they will become effective.

It is evident that, among the constituents of mathematical ability, the powers of analysis, synthesis, abstraction and concrete imagination hold a prominent place. It has been suggested that these boys seem to excel the girls often in synthesis and generally in concrete imagination. In this experiment, where the several factors were given equal place in the tests as far as possible, the superiority of the boys in one or two tests did not effect a marked sex-difference in the whole. But in school mathematics, where the above factors may be assumed to permeate the whole to a much greater extent, the sex-difference effected will be more marked.

An experiment of this kind naturally raises more questions than it answers. Even the answers it has attempted need further confirmatory evidence. Similar tests ought to be applied in a much greater number of schools, not only to obtain this evidence, but also to subject such tests to the light of experience. Experimental work of this kind is yet in its infancy. Perhaps the difficulties revealed, the problems raised and the solutions attempted may be of some use in further investigation.

Thanks are due to Prof. T. P. Nunn for kindly reading this MS. before publication, to Dr W. G. Sleight for his helpful suggestions, to Miss O. D. Rudkin, M.A., for assisting in the scoring and for checking the calculations, and to the Headmasters and Headmistresses for their kindness and courtesy in allowing the tests to be applied in their schools.

(*Manuscript received* 12 *September* 1924.)

A NOTE ON THE 'PSYCHOGALVANIC' REACTIONS OF ANAESTHETISED CATS

By F. AVELING and R. J. S. McDOWALL.

(*From the Physiological and Psychological Laboratories, University of London, King's College.*)

In order further to determine the significance for psychology of the 'Psychogalvanic Reflex,' experiments were carried out, with the assistance of Rev. R. C. McCarthy and Miss H. M. Wells, on anaesthetised cats.

The apparatus used was a Wheatstone-bridge resistance box, made to the design of the late Dr Waller; an Onwood moving coil galvanometer (resistance, 100 ohms; sensitivity, 330 mm. per micro ampere at one metre scale distance; period of deflection, 4–5 seconds when movement of coil is well damped); lamp and scale. Zinc electrodes covered with wash-leather soaked in a 0·9 per cent. saline solution were bound with surgical bandages to the pads of the hind paws of the animals, which formed the resistance in one of the arms of the bridge. The electrodes were also applied in other experiments to the skin, from which the hair had been clipped, with similar results to those noted below.

The cats were anaesthetised in the first place with ether, and afterwards by an intravenous injection of chloralose. Continuous blood-pressure records were taken in the case of several of the animals from the left carotid synchronously with the galvanometer readings. All injections of drugs were made in the right femoral vein. In the case of several animals both carotids were ligatured and both vagi transected. Several had neither carotid tied and the vagi were intact. One animal was decerebrate, having all centres down to the stalk and cerebellum ablated. In all these instances deflections of the galvanometer were obtained, following on the application of 'nocive' stimuli and the administration of drugs. But successive applications of the same stimulus gave lesser, or no, deflections and large doses of drugs lessened, or abolished, the deflection for subsequent doses of the same drug, or of another having similar effects with regard to the constriction or dilatation of blood vessels.

The following 'nocive' stimuli were applied with the results noted:

Stimulus	Effect
1. Skin on thorax cut	Fall in resistance
2. Forced flexion of forepaw	Fall in resistance
3. Gas pliers closed strongly on foreleg	Marked fall, followed by slight recovery after withdrawal of stimulus
4. Saline very *hot* water poured on wound in thigh	Fall in resistance
5. Ice applied to carotid or to wound	Fall in resistance
6. Warm saline solution injected into carotid	Rise in resistance
7. Haemorrhage (10 c.c. blood)	Marked fall in resistance
8. This blood was then whipped and 7 c.c. injected into femoral vein	Slight rise in resistance
9. Vena cava clamped	Slight fall, followed by very marked rise in resistance
10. Asphyxia during 132 seconds	Very marked rise in resistance, preceded by a slight fall.
11. Forced artificial breathing	Small fall in resistance

The skin of the animal was stripped locally and the electrodes applied to the subcutaneous tissue, with the following results.

1. The normal resistance was found to be very low in comparison with that through the pads (about 2,600 ohms, as against 9,000–65,000 ohms).

2. 'Nocive' stimuli (flexion of paw, pinching of skin on thorax, asphyxia) produced results similar to those obtained when the electrodes were applied to the pads; but the deflections were less pronounced.

The following drugs were injected into the animals with the results noted:

1. Adrenalin $\frac{1}{50}$ mg. Fall in resistance, very gradual at first, then rapid; synchronous with rise in blood pressure.
2. Acetyl Choline ·001 mg. Rise in resistance, synchronous with fall in blood pressure. Both recovered synchronously.
3. Atropine (large dose). Rise in resistance. No recovery over a long period.
4. Amyl Nitrite (administered by inhalation). Rise in resistance.
5. Pilocarpine 1 mg. Fall in resistance synchronous with fall of blood pressure. This was followed by a rise in resistance after about 5 minutes. Synchronously with this rise the pads of the animal began to sweat profusely.

From the known action of these drugs upon the blood vessels it is evident that a fall of skin resistance accompanies a constriction of vessels in the skin, subcutaneous tissue and muscle. Further, the mechanical stimuli (included among 'nocive' stimuli above) which produced a fall in resistance are those which also effect a constriction, while those which produced a rise also effected a dilatation of vessels. In the papers referred to below it has been argued that the 'P.G.R.' indicates the action of a 'Skin Constrictor Reflex.' This seems to be what is principally measured by the deflections which are obtained by the application of 'nocive' stimuli when the electrodes are fixed to the palmar and dorsal surfaces of the hand.

Considering the results of this investigation, together with the effects of 'nocive' stimulation in human beings—and, in particular, of *threats* of 'nocive' stimuli—we draw the following conclusions:

1. The fact that deflections are obtained even when the skin is removed is evidence against the theory that they are primarily caused by secretion of the sweat glands. This is corroborated by the action of cold and pilocarpine. Atropine in small doses did not abolish the reflex.
2. If we are to hold that 'emotions' are correlated with the functioning of the thalamus, or basal ganglia, the results in the case of decerebrate, or equivalently decerebrate, preparations show that the reaction is not 'emotional.'
3. 'Nocive' stimuli provoke reactions by means of which the organism is fitted, or 'set,' for coping with a threatening situation. These reactions are to be considered conative rather than emotional. They need in no sense be cognitive.
4. Since *threats* of 'nocive' stimuli cause deflections of the same kind as those studied in this investigation, it appears certain that such threats provoke defensive organic 'sets' of a conative character, and that the reflex is primarily an indication of conative organic reaction, conscious or unconscious.
5. The term 'Psychogalvanic Reflex' is therefore, from the point of view of nomenclature, too specific. Perhaps 'Orectic Reflex' would better express the facts.

Papers on the more physiological aspects of the experiments appear in the *Proceedings of the Royal Society* and *Journal of Physiology.*

[The expenses of the research were assisted by a grant from the Governments Grant Committee of the Royal Society.]

(*Manuscript received* 7 *April* 1925.)

ON THE INTUITIVE CAPACITY OF CHILDREN TO UNDERSTAND SPOKEN LANGUAGE

BY J. W. TOMB.

No one living in a country such as India, where many different languages are spoken, can fail to be struck with the great facility with which European children pick up the many languages which they hear spoken around them, whilst their parents as a rule speak only one of these languages and that very imperfectly. To put forward a hypothesis to explain this striking state of affairs is the object of the present article.

It is commonly assumed that children learn a language as adults do, by the conscious memorising of parts of speech, moods, tenses, etc., but a moment's reflection will show that this cannot be so in the ordinary case of infants learning their mother-tongue, the parts of speech, moods, tenses, etc. of which cannot be consciously apprehended by them as such; or their use explained to them through the medium of language since they possess none. Moreover, children are universally able to converse freely in their mother-tongue without possessing any appreciation of its grammatical construction, which in civilised countries they afterwards have to learn by tedious years of study at school. Infants, therefore, must possess the capacity of intuitively bestowing meanings on the sounds (*i.e.* words) used in their hearing by their nurses, mothers, relatives, and other people—a power which as they grow up to adult life they subsequently lose.

It is a common experience in the district in Bengal in which the writer resides to hear English children of three or four years old who have been born in the country conversing freely at different times with their parents in English, with their *ayahs* (nurses) in Bengali, with the garden-coolies in Santali, and with the house-servants in Hindustani, while their parents have learnt with the aid of a *munshi* (teacher) and much laborious effort just sufficient Hindustani to comprehend what the house-servants are saying (provided they do not speak too quickly) and to issue simple orders to them connected with domestic affairs. It is even not unusual to see English parents in India unable to understand what their servants are saying to them in Hindustani, and being driven in consequence to bring along an English child of four or five years old, if available, to act as interpreter.

The writer recently became acquainted with a very interesting case of an English girl child aged four who had a Tamil-speaking *ayah* with her for two months only, her permanent *ayah*, a Bengali, being ill. Within that period of time and without any apparent effort, the child was able to understand what the *ayah* was saying to her in Tamil and to converse with her in that language. It was moreover observed that the child even continued to speak occasionally in Tamil to the other servants for some time after her Tamil-speaking *ayah* had gone away, apparently not recognising that she was speaking to them in a language which they did not understand. Such a case as this can be satisfactorily explained only on the hypothesis that the child 'intuitively' and subconsciously placed the correct meanings on the various sounds (words) in Tamil which the *ayah* used to her in conversation, and appeared unaware of any difference between the various languages (English, Bengali, Santali, Hindustani, Tamil) which she was able to speak.

But perhaps the truth of the hypothesis is most apparent in the case of infants who are not yet able to speak any language, for in this district one frequently sees and hears English babies of from nine months to eighteen months old receiving directions and instructions from their mothers in English, from their *ayahs* in Bengali, from the house-servants in Hindustani, etc., and obviously understanding what is being said to them in each and all of these languages long before such infants are able to speak any of these languages themselves. It is also observable that infants appear to comprehend languages in whole sentences which they endeavour to reproduce *in extenso*, but owing to the undeveloped condition of their cerebral cortex and particularly of the speech centre, are unable to do so, merely as a rule producing some simple and easily articulated word belonging to the whole.

The writer has also observed many instances where English children of three or four years of age have been brought out to India by their parents, who themselves have come to India for the first time; such children after one year have in all instances acquired the capacity both to understand and to speak freely in all the vernaculars of the district they live in, whereas their parents, unless they have employed a teacher and deliberately studied one of the languages, are quite unable at the end of that time to understand what is said in any of them, and are only able to use a few essential and disconnected words, such as those for water, milk, food, chair, table, bed, etc. It is obvious, therefore, that a child's capacity for learning languages is of a subconscious nature and does not depend on 'intelligence,' for if the latter were the case it is reasonable to assume that in the instance referred to the parents would pick up the various vernaculars more quickly than their children, whereas experience shows that the direct opposite occurs. Children must therefore possess a capacity, not based on 'intelligence,' of intuitively placing the correct meanings on spoken sounds, a capacity which they retain up to a certain age, but, in the majority of instances, lose altogether as they approach adult life. On no other hypothesis can the facts related above be intelligibly explained. The writer would welcome criticism of these views by others who, like himself, have had an opportunity of studying how children in foreign countries pick up the language or languages spoken around them there.

(*Manuscript received* 15 *April* 1925.)

CRITICAL NOTICE

By CHARLES FOX.

The Growth of the Mind. An Introduction to Child-psychology. By K. Koffka (translated by R. M. Ogden). Kegan Paul (1924). Pp. xvi + 383. International Library of Psychology, Philosophy and Scientific Method.

This book is a consistent attempt to apply the hypothesis of the *Gestalt* psychology over the whole range of mental growth, and thereby to deal with the chief problems of educational psychology. Its main thesis is summed up in the last sentence of the volume: "The nature of mental development...is not the bringing together of separate elements but the arousal and perfection of more and more complicated configurations in which both the phenomena of consciousness and the functions of the organism go hand in hand." The thesis is cleverly developed in all fields of developing experience; sensation, movement, instincts, ideation, are all in turn brought into the configurative net, and since the period of infancy is largely spent in play this too is dealt with *sub specie structurae.* It must be admitted that the method of treatment leads to many fruitful analogies and illuminates several questions of child development.

The *Gestalt-Theorie* starts with the sound doctrine of a form, shape or configuration in all mental content in opposition to mental atomism, which assumed a discrete manifold of presentations somehow reduced to form or order by apperceptive synthesis. Consequently Professor Koffka devotes a good deal of space to an attack upon the American school, led by Professor Thorndike, which regards all mental development as due to the formation of bonds of association between sensation and movement. Apparently, the author is quite oblivious of the fact that British psychology has for over a quarter of a century put off its mourning garb for associationism. Professor Ward[1] gave mental atomism its quietus long ago, as may be inferred from the following pregnant sentence: "At its first appearance in psychical life a new sensation or so-called elementary presentation is really only a partial modification of some pre-existing and persisting presentational whole, which thereby becomes more complex than it was before; and this increasing complexity and differentiation never gives rise to a plurality of discontinuous presentations." When therefore the skeleton of associationism was dug up in America and the attempt made to show that by imposing 'movements' on it in place of the old 'ideas' it could be resuscitated and 'behave' as though it breathed the breath of mental life the British school was naturally sceptical at such a resurrection.

---

[1] *Psychological Principles,* ch. 4, par. 2.

Instead of a chaos of disconnected impressions subsequently reduced to order we assume that a certain arrangement dominates conscious life from the very beginning. The first phenomena which a child experiences are comparable to the discrimination of the separate notes within a chord, the rest of the chord consisting as yet of an undifferentiated volume of sound awaiting further analysis. The undifferentiated presentational background is itself part of what is experienced and it is useful to have a name for the relation of the parts to the "pre-existing and persisting presentational whole." Such a co-existence of phenomena in which each member is dependent for its significance on its relations to the whole is called a *struktur* or configuration. In this sense all our experiences are figured or shaped, even the most primitive, since the earliest impressions are structurally related to the presentational continuum, which itself has form owing to the fact of heredity. Psychology is constantly hampered by adopting the metaphysical standpoint. Whatever is true epistemologically, if we adhere to the psychological point of view we are forced to conclude that all the categories of perception as, for instance, the Unity of Attributes in a single Subject are given in the most primitive experience. "From the outset," says Professor Stout[1], "a given attribute or given attributes are implicitly apprehended as *parts* of the nature of a thing and not as its whole nature, *so that the mind is prepared to look for further attributes* whenever occasion arises and interest prompts it to do so." (Italics mine.) There you have the whole *Gestalt* theory of the perception of 'things' in a nutshell. With this sound psychological doctrine the *gestaltists* combine a mystical belief in a pre-established harmony between the course of our thinking and the events in the world, together with a better founded refusal to explain mental connections by means of pathways in the brain.

The *Gestalt* theory is very fond of musical analogies, which do in fact help to elucidate the problems of mental growth. Thus Professor Koffka says "The new-born infant experiences the world differently from us adults, just as an unmusical person hears a symphony differently from one who is musical." The difference consists of a greater insight into the structure or configuration of the symphony, and all intellectual advance may be conceived as an intuition of the harmonies which the world offers for our appreciation. Take the case of forming a new habit. It is too readily assumed that this is due to the welding together of a chain of neurones uniting afferent and efferent processes, so that the whole movement becomes stereotyped. But a little observation will convince anyone that habits may be mechanical without being stereotyped. Consider what happens when I sign my name, which is about as fixed a habit as I have. Sometimes the paper on which I write is to the left, sometimes to the right of me, sometimes pushed forward and again near; and in every case a more or less different movement is required. Further, I may write with a pen on paper or with chalk on a board, when it is obvious even to the most casual observer that a totally different set of muscles is employed, for in the latter case I write with my arm and not with my hand. Yet every time I produce the same signature. What, then, becomes of the fixed chain of intercentral neurones; or are there several chains each appropriate to a different set of circumstances? The difficulty is accentuated when it is considered that if I write simultaneously and unreflectingly with both hands the left hand produces a mirror image of the right-hand movements. Are we to suppose that the establishment of the right-hand chain of intercentral neurone paths led originally to the formation of an equivalent chain for the left hand?

---

[1] *Manual*, Bk. III, Part II, ch. 1.

Difficulties such as these, which are usually glossed over in describing habit-formation, lead one to doubt whether there are fixed nervous connections between the sensory and motor sides of the process. It is more in consonance with the actual facts to say that what I am trying to do on each occasion, when I sign my name, is to adapt my movements to a somewhat similar sensory configuration. The activities constituting the act of writing are not comparable to a series of disconnected notes, but to a melody which may be played in different keys. Why then, it may be asked, is repetition necessary to form a habit unless we are blazing a trail through a forest of neurones in order to make a permanent pathway? The answer is that mere repetition is useless. If one tries to learn a set of nonsense syllables by bare repetition, keeping the mind as passive as possible, the attempt is well-nigh hopeless. It is only successful, if at all, because it is impossible to keep the mind perfectly passive. Moreover, if success is attained, the series is rapidly and completely forgotten[1], since there is little difference between the number of repetitions required for re-learning what has once been learnt passively and the number of repetitions required to learn for the first time. After such a series of syllables is learnt with determination it is never completely forgotten, as the well-known logarithmic law of Ebbinghaus indicates. On the assumption of the linking up of a chain of neurones such results are inexplicable; for as far as the movements of the muscles are concerned it is immaterial whether we repeat the series with or without the desire to learn. The function of repetition in habit-formation is not to establish paths of low resistance, but to adjust the muscular system to the appropriate configuration, just as a violin only yields the best tone after prolonged use. Learning a habit is, in brief, attuning the muscles to form the appropriate movement-melody. The motor neurones respond to the sensory not because they are physiologically connected by intraneural paths, but because together they constitute a unitary physiological organ in which the parts function in unison. When such functioning is due mainly to maturation we are dealing with instinctive activities, but when it requires prolonged learning the action is called a habit.

It is useless to turn to reflex actions in the hope of finding a safe footing for intracentral nervous conduction paths with predetermined functions. For consider the movements of fixation of the eye. The actual motion made depends not only on the particular part of the retina stimulated, for which it is possible that a definite (though an uncomfortably large) number of central connections are provided, but also on the original position of the eyes with respect to the orbit; which presupposes that each retinal point must be connected with an indefinitely large number of central neurones. Perhaps the microscope will assist where mathematics fails and the enormous number of dendrites for each nerve cell be prayed in aid. Unfortunately for this hope experimental evidence bars the way. Marina, experimenting on apes, interchanged the superior rectus muscle of the eye for the lateral rectus. After the wound was healed the animal ought to have made the most astonishing eye movements in the endeavour to fixate points in his field of vision and to save the neurone theory of connections from disaster. Instead of which both voluntary and automatic movements were carried out appropriately, and Marina concluded that "the anatomical association-pathways from the centres to the eye muscles are not fixed." And we had better conclude that it is simpler to suppose that the movements of fixation, like habitual movements, are determined to a very considerable extent by the configuration of the visual phenomena involved.

---

[1] See "Experiments in learning, etc.," Smith and McDougall, this *Journal*, vol. x.

If the notion of intracentral neurones connecting afferent and efferent paths is given up we shall have to abandon the idea that a sensation is determined by its stimulus. Here we approach a fundamental problem in educational psychology, namely, whether the effects of training can be transferred. The work of Köhler is of prime importance in this connection. An animal is presented with two stimuli, *e.g.* a darker (negative) and a brighter (positive) grey box and is trained to choose the positive box which contains food, until he is completely successful. Thereupon, at a later time, two boxes are shown to him, one being the original stimulus, namely, the positive box, and the other a still brighter grey box (neutral). If the animal responds to the stimulus *colour* grey we should expect the positive box to be chosen as the result of training. But this is by no means the case, for the neutral colour was chosen even by stupid hens more often than the positive; our intelligent friends, the apes, chose the neutral tint practically every time and a child invariably selected the neutral grey. Thus, the response is made not primarily to the colour sensation but to the configuration or pattern of the stimulus—in this instance to the relation 'brighter than.' If metaphysicians were ever convincible by facts or argument such observations as these ought to lead them to abandon the view that there are things like patches of colour or chunks of isolated sound waiting about ready to be snapped up as sense-data. Even a hen refuses to pick them up. Such epistemological *débris* may serve to construct a metaphysician's world but must be swept aside by the student of psychology who wishes to understand a living mind.

Educational psychologists, unlike the intelligent apes and the child, choose the view that transference of the effects of training is limited to those cases where there are identical elements. On Köhler's view identical elements are ineffective and transfer takes place because the individual acquires insight into the relations of a configuration. The stimulus pipes a relational tune and the individual dances in the correct relational rhythm. These and like experiments on human adults show that unless the task is accomplished with insight into relationships the effects of training are not transferred. There was therefore a good deal to be said for the old classical training, which rested on the theory that mental discipline resulted in a *general* flexibility of mind, since the relations of a configuration are universal. The chief objection to the classical grind was that so few acquired the discipline, since insight was rarely attained owing to the lack of interest in the preliminary drudgery.

Much light is shed on the problem of imitation by the hypothesis of *Gestalt*. We have seen that it is difficult to allow that habit-formation is the result of making paths of low resistance between the sensory and motor constituents of a habit; rather there is a unitary organ, the sensorium-motorium, whose parts function in unison. Now, imitation, in which the child is an accomplished master, is usually said to consist either of imitation of movements or of the ends attained by them, but it is hard to find unimpeachable cases of the former kind of imitation in children. When a child runs because he sees another doing so, what he imitates is the *running* and by no means the leg and trunk movements of his fellow. This is exceedingly unfortunate for a dull world, since when he sees a corpulent uncle trying frantically to run, he might cause considerable amusement by attempting to imitate his movements. When I pull out my watch because another person pulls out his, what I imitate is his purpose or end, for my watch may be in a different pocket and unattached to a chain; in fact I am not interested in his movements at all, but simply in the end attained. An imitated movement is thus never a photographic copy, but simply a reproduction of a similar configuration. The true nature of imitation is seen when I imitate a melody on a violin which I hear on a piano. We do not require a system of similar afferent and efferent impulses to account for imitation or to assume that there is an instinct of imitation founded on nervous pathways. "It may be," says Professor McDougall[1], "that every child inherits a considerable number of rudimentary instincts and that they play a considerable part in facilitating the acquisition of new movements, especially perhaps of speech movements." This is based on the view that imitation is "the copying by one individual of the actions, the bodily movements, of another." But this is never done; and what an amazing number of afferent paths would be required to account for all the nuances of consonant and vowel sounds in all their living complexity! Only the trained phonetician gets within measurable sight of copying the speech movements of another person and he uses elaborate diagrams and apparatus, and even then experts fail to agree on the copy; but the surprisingly clever baby does all this without effort by means of nervous paths.

It may perhaps be thought that the sub-title of the book should be "abandon hope all ye who rely on nervous connections." The final chapter is the most suggestive and should certainly be studied by teachers who have to deal with infants. It is concerned with the world of a child and the author very cleverly applies the *Gestalt* hypothesis to this region. The adult's world gradually encroaches on the territory of the child's play world by the constant pressure of education. At first, configurations in the two worlds are independent. A child is perfectly content to treat a stick as a horse though he is aware that his father treats it as a curious walking implement. When the stick enters the play world it possesses for the child the essential characteristics of a horse. A delicious remark, recorded by Sully, strikingly illustrates the different points of view. "One day two sisters said to one another: 'Let us play at being sisters.'" These child geniuses meant to take over into the play world the configuration of sisterdom, with all the delight appertaining to freedom from the constraint of the adult world. One of the supreme joys of the play world lies in its independence of the laws of uniformity and contradiction; a doll can be in different places at the same time and have different personalities and yet be the same. A similar independence pervades the magic world of primitive man.

---

[1] *Social Psychology*, ch. 4.

Finally, it should be said that the problem of learning with insight, which is an essential part of the *Gestalt* theory as opposed to the principle of learning by trial and error, is ably dealt with; but this section of the book is based entirely on Köhler's work and can be more fully studied in the latter's *Mentality of Apes* recently published in the International Library of Psychology, etc., by the publishers of this book. No student of educational psychology can afford to ignore Professor Koffka's stimulating treatise.

PUBLICATIONS RECENTLY RECEIVED

Principles of Literary Criticism. By I. A. RICHARDS. London: Kegan Paul. 1925. pp. vi + 290.

This work is to be welcomed as a systematic attempt to provide a psychological basis for aesthetic criticism. Its subject is conceived in a liberal sense to include the field of the arts generally, musical, pictorial and plastic, as well as literary. At the outset the author draws two distinctions of considerable importance for his subsequent discussion. The first relates to the separation, in any complete critical statement, of the *technical* part, describing the qualities of the object which give rise to a valuable experience, from the *critical* part, which deals with that experience and with its assigned value; it is with this latter part, regarded as essentially a branch of psychology, that the present book is mainly concerned. The second distinction is that between the communicative efficacy of a work of art and the value of the experience which it thereby conveys. As might be expected, it is this question of value which forms the pivot of the author's argument; and his theory here may be briefly indicated. The only things which are good or valuable are the exercise of impulses and the satisfaction of their appetencies; for on this view 'good' simply *means* that which satisfies, and a good experience *is* just "one in which the impulses which make it are fulfilled and successful," provided that their satisfaction entails a minimum of conflict and interference with other competing impulses. Of any two systems of experience that one is the more valuable which is the more stable, that is, in which the component impulses are so organised that the greatest range of possible impulses finds expression, with the least consequent waste and sacrifice. We can then state in terms of this view what is meant by a good poem, or a good play, or in general a valuable work of art: the goodness of a work of art depends on the value of the experience embodied in it by its creator and the efficacy with which the work in question communicates that experience to others. There may be deficiency in either or in both of these respects; but a sonnet or a symphony which is lacking in communicative efficacy need not be devoid of value, and one which has a more catholic appeal than another is not necessarily a greater work of art.

Few readers, we think, will quarrel with the author in his belief that there is room and need for the kind of enquiry which he has here undertaken. But if Mr Richards has, in the main, chosen the right questions, some of his answers leave a good deal to be desired. Space will not permit consideration of all the issues which he raises, and we must be content here with a brief notice of two of them, viz. his theories of value and of knowing or awareness. As regards the first, Mr Richards professes to give us a *psychological* theory of value; and by this he appears to understand a theory which reduces the notion of value to purely psychological terms. But we must confess that we have failed altogether to see how this reduction is supposed to be effected. It seems to us that in the sense of the term which is appropriate here there can be no psychological theory of *value*, but only of *valuing*; for in this sense of the term *value* is not a psychological concept at all. With this view our author has apparently little sympathy. It is not, however, to be disposed of by superficial references to 'abstractionism' and 'premature ultimates'; and it seems clear that the author's own explicit contentions can be rendered plausible only by its tacit assumption. What gives an experience value, he tells us (p. 132), is the "organisation of its impulses for freedom and fulness of life." But by 'value' here cannot be *meant* simply some organisation of this kind; the statement is not put forth as a mere tautology, and in any case it is beyond question that *other* things than this have been held by many people to possess value. Not only so, the question whether any such organisation itself has value depends on the sort of organisation which it is, and on the nature of the impulses which it suppresses and combines. And thus we are eventually forced, by implication at least, to appeal to that very ethical conception of value which was alleged to be superfluous. Psychology is not indeed without its function here. But that function is not to 'reduce' the categories of ethics to its own; it is (in the present case) to provide us with an account of the nature and conditions of the experience or process of valuing—an account towards which Mr Richards himself makes useful contribution.

To pass now to the second point. For the "knowing, feeling, and willing aspects of a mental event," Mr Richards proposes to substitute the causation, the character, and the consequences of the event (p. 89). Thus, on this view, to say that a mental (neural?) event is aware of various black marks on this page is to say that "it is caused by them, and here 'aware of' = 'caused by.' The two statements are merely alternative formulations" (p. 90). Now it seems plain from the context that the author does not mean this to be a mere statement as to how he is going to use the words 'awareness of,' 'thought of,' or 'knowledge of'; it is avowedly what claims to be in principle an adequate psychological account of the process ordinarily called 'knowing.' But surely such a view, on the very face of it, declares its own bankruptcy. The mind, we are naïvely informed, *is* the nervous system, or rather a part of its activity (p. 83); as though this facile identification provided psychology with an answer to its question. To assume that what is true of neural events is true *ipso facto* of mental events is to offer us a flagrant *petitio* for the solution of a problem. The original issue is left precisely where it was.

So many topics are touched upon within the modest compass of this book that the treatment of many of them is perforce slight; and a much fuller discussion of such subjects as those of Imagination and Memory would have been helpful. On the other hand, despite their brevity, some of Mr Richard's chapters—*e.g.* those on rhythm and metre and on the emotive and scientific uses of language—strike us as excellent. Mr Richards is an entertaining writer whose work avoids easily the tedium of a technical treatise. His pages are stimulating and suggestive, and enlivened frequently with judicious and apposite allusion.

W. H. O'N. M.

Instinct, Intelligence and Character: An Educational Psychology. By Prof. GODFREY H. THOMSON. London: George Allen & Unwin, Ltd. 1925. pp. 282. 10*s.* 6*d.* net.

It is stated on the cover that this book is for "teacher, parent, student, and man in the street." For it, the teacher, parent and man in the street should be grateful. It is lucid and interesting without being superficial. This means that it is an ideal popular presentation of psychology. Perhaps the idea still persists in some quarters that it is easy for a man of science to write a popular book on his own subject and that it is little worth doing. Certainly it is not easy, and few living psychologists could do it as well as does Professor Thomson. The work of bringing the results of modern scientific research within reach of those outside is, too, a work whose importance is not much less than that of discovering new truth. The man in the street who has read this book will have acquired a background of solid psychological theory, and will know much that has hitherto been inaccessible to him except in technical journals. He will, for example, have been taught by ingenious analogies and simple explanations the functions of imagery, the methods of measuring intelligence, and the latest work on formal training and on cerebral localisation.

For the student (at least, the honours student of psychology) the book is necessarily less suitable, for references to the actual places of publication of original work are rare and the author refuses to complicate his presentation by going into details of rival points of view. The requirements of the student are, in fact, incompatible with those of the teacher, parent, and man in the street, and it is for the latter group that Professor Thomson has written.

This book should prove particularly valuable for teachers. Since it is probable that, in the future, psychological tests will be increasingly used in schools, the needs for such an adequate account of psychological theory will increase. Another class of readers amongst whom Professor Thomson's book should prove welcome are those whose business it is to teach psychology who feel the need for an object lesson in clear presentation.

R. H. T.

The Bilingual Problem: A Study based upon Experiments and Observations in Wales. By D. I. SAER, FRANK SMITH, JOHN HUGHES, University College of Wales, Aberystwith. 1924. pp. 112. Price 2*s.* 6*d.*

This short, but very interesting monograph is an expansion of two papers published in this JOURNAL in 1923. It deals directly only with the effects of bilingualism in Wales, but obviously the study is one which has significance in many parts of the British Empire. The authors first describe how the day school in Wales developed into "an overwhelmingly English institution." They then proceed to show that there is much evidence that "the child who learns two languages at an early age possesses mental characteristics peculiar to himself," and that these differences are more marked among rural than among urban children. These suggestions, and the details into which they are worked, are put forward with admirable caution. The authors call for further, more extensive, and more completely controlled research.

The essay is clear and altogether to the point. It contains a useful bibliography, and it should certainly be consulted by every teacher who has to face problems of the type dealt with.

Lefthandedness: A New Interpretation. By B. S. PARSON. New York: The Macmillan Co. 1924. pp. vii + 185.

Whatever may be thought of the theory here put forward, this book is undoubtedly a very interesting one. The author passes in review most, if not all, of the theories that have been advanced to explain 'handedness'—for the title *left* handedness is hardly a fair indication of the contents. He then carefully but concisely explains his theory that the dominant use of one hand depends upon uniocular sighting, and describes the tests by which he considers his view to be supported. In binocular vision, he holds, we must believe that the images of each eye reach consciousness separately, and that "in unilateral sighting, as in strabismus, one image is suppressed or disregarded." This view, as the author himself points out, makes right- or left-handedness to be a psychological rather than a physiological function.

The book is modestly and attractively written. There is a very excellent bibliography.

Mental Measurement in Educational and Vocational Guidance. By JOHN M. BREWER and others. Harvard Bulletins in Education. Cambridge, Mass. Harvard University (Oxford University Press, London). pp. vii + 46. Price 2*s*. 6*d*. net.

This is a brief summary and interpretation of the work so far done upon mental measurement in relation to educational and vocational guidance. It deals with English and American work only, and should be useful to any general reader who wishes to obtain a rapid survey of the field concerned.

The Permanence of the Vocational Interests of Junior High School Pupils. Johns Hopkins University Studies in Education, No. 8. By Dr E. E. FRANKLIN. Baltimore: The Johns Hopkins Press. 1924. pp. 63. Price $1·15.

This extremely interesting short monograph deals with a problem which is obviously both profoundly important and difficult of solution. Dr Franklin has worked ably, and presents his methods and conclusions clearly. The latter tend to show a greater permanence of interest than might have been expected, particularly among girls. It also appears that "the vocational interests expressed by the pupils are vital and practical." There is much need that work of the same kind should be carried out in this country. To any student who proposes to undertake such a task, this study may be recommended.

An Introduction to Psychology. By Dr H. A. REYBURN. Cape Town: Maskew Miller, Ltd (London: Geo. Harrap & Sons, 7*s*. 6*d*. net.).

This is the first definite text-book on psychology to come from South Africa. It is clearly and simply written. Its standpoint is modern and functional, though it carefully avoids the extremes of behaviourism. On the whole it cannot be said to be very original either in thought or arrangement, but it is certainly lucid and it has also the great merit of being interesting.

Special Talents and Defects: Their Significance for Education. By Dr LETA S. HOLLINGWORTH. New York: The Macmillan Co. 1923. pp. xv + 216. Price 7*s*. 6*d*. net.

After a general consideration of the nature of ability, a study of the results of research so far carried out upon the relationship of capacities, and a discussion of the possible neural basis of intelligent reaction, Dr Hollingworth proceeds to deal in turn with a number of special abilities useful in school work. She thus deals with

Reading, Spelling, Arithmetic, Drawing, Music, and a few miscellaneous capacities, such as left-handedness, mechanical ability, and leadership. The final chapter deals with the primary importance of the individual in education.

Although the book as a whole is rather of the nature of a compilation, it was well worth attempting. The author has no difficulty in showing how significant a study of individual abilities and disabilities may be. The evidence which she cites is marshalled with much skill, but all the time it is obvious that further research is called for. The chapter on "The Neural Basis" probably could not have been very good in any case. As it is, however, it is so slight that it might well have been omitted.

The Story of Man's Mind: The Psychology of Business, Home and School with its Thousand Uses and Applications Explained for Everyone. By GEORGE HUMPHREY. London: Geo. Routledge & Sons. 1925. pp. ix + 302. Price 10*s.* 6*d.* net.

Mr Humphrey tells us, as the publishers say: How to train the baby, what dreams mean, what advertisements appeal, why we shiver in a hot bath, why a child fears the dark, why we have dislikes, what psycho-analysis is, how to cure insomnia, why we love, how we remember. The chapter- and subheadings are rather like the descriptive tags of a cinematograph film: "Does a jelly-fish think?" "Man, marionet of his surroundings," "Victor, the boy who lived among the animals," and so on. Nearly every question he introduces with a story. The sentences are short, the paragraphs snappy. The whole is a remarkable exercise in popularisation, which, it is to be hoped, could hardly go further. Yet the book is undeniably well done. There is much true observation, much condensation of genuine psychological experimentalism. And in the end the author hands on to his reader a number of references to more serious books than his own. He calls them, indeed: "Thirty-seven Interesting Books on Psychology." But the people who like this very much, will probably find the favoured "Thirty-seven" rather dull.

Skill in Work and Play. By Prof. T. H. PEAR. London: Methuen & Co. 1924. pp. 107. Price 4*s.* net.

This book contains six lectures dealing with: The description of muscular experience; the acquisition of skill; the experimental investigation of learning; training in muscular performances; the relation between training and education; the training of the intermediate ranks in industry. The bent of the book is practical, although a number of points of theoretical importance—such as the significance of 'plateaus' in learning, and the significance of varieties of imagery in relation to type of muscular reaction—are raised. The exposition is popular in style, but it is exceedingly clear and interesting, and is throughout well informed. In fact the book does what it professes to do remarkably well. The general reader who peruses it will be interested and informed, and the psychologist will find a number of problems raised which may give him material for experimental research.

The Measurement of General Ability: An Australian Revision and Extension of the Binet-Simon Scale. By Dr G. E. PHILLIPS. Sydney: Angus & Robertson. 1924. pp. 119.

In this small book the results of over 1500 Binet records obtained from testing Australian school-children are utilised. The revised scale is printed, and brief but clear directions are given for its application. The amount of labour necessary for the production of a report of this kind is frequently given but slight recognition. The author and his colleagues are to be congratulated upon the excellent manner in which they have carried out a prolonged and laborious task.

Proceedings of the VIIth International Congress of Psychology held at Oxford in 1923. Edited by C. S. MYERS. Cambridge University Press. 1924. pp. xxv + 388. Price 12*s*. 6*d*. net.

This remarkably well-produced volume contains not only the papers read at the Oxford Congress but also a brief account of the various meetings and a list of those who attended as members. A number, but not all, of the papers have already been published elsewhere, particularly in the pages of this JOURNAL. It is, however, especially interesting to have them all collected together into one volume. The work is an impressive testimony to the width of interests, the vitality and the deep interest of contemporary psychology.

Moderne Willenstheorien: Eine Darstellung und Kritik. Von EINER TEGEN. Upsala: A.-B. Lundequistska Bokhandelp. S. 309. 12 kr.

In this first volume of the author's projected review the works of Spencer, Bain, Ribot, Lotze, Sigwart and Wundt are all dealt with. Sigwart and Wundt are accorded far more space than the others, and Wundt in particular is dealt with in very considerable detail. Much attention is given to Wundt's doctrine of apperception in relation to volition, and to his views as to the organic foundation of voluntary activity. The author considers that these are, on the whole, less original than might appear.

Blinden-Psychologie. Von Dr K. BÜRKLEN. Leipzig: Johann Ambrosius Barth. 1924. S. 334. G.M. 10.80/12.80.

Dr Bürklen has produced a comprehensive and yet a concise treatise on the psychology of the blind. He deals, in successive parts, with sensation; perception; memory, attention, imagination and thinking; feeling and volition, and the general effect of blindness upon the whole personality. He adds a very useful bibliography, including however only works in the German language. The exposition is clear, well-informed and full of interesting material, though here and there it presents rather the appearance of a compilation from other writers' work. The parts which deal with touch perception, and with the apprehension of spatial relation in general are especially worth reading.

Some Problems in Mental Anthropology and the Problem of Civilisation. By Captain G. PITT-RIVERS. Wellington, N.Z.: W. A. G. Skinner. 1924.

This is the Presidential Address delivered in 1923 by Captain G. Pitt-Rivers to the Ethnological and Anthropological section of the Australasian Association for the Advancement of Science. It deals in general with disintegrative and integrative forces which are to be found in the primitive community. Three of the latter are dealt with in some detail and with numerous illustrations: chieftainship, magic and sorcery, and systems of exchange and gifts. The author's general thesis is that a community is healthy in proportion as it is amenable to integrative influences. The psychologist will find much of interest, not only in the general trend of Captain Pitt-Rivers's argument; but particularly in many of the illustrations which he brings forward.

Law and Freedom in the School. By George A. Coe. University of Chicago Press. 1924. pp. ix + 133. Price $1.75.

According to the publishers, this book is of "high inspirational quality to the teaching profession." It is, in a way, a reaction against the current desire to allow to children unfettered activities, a plea that all activities, to be useful, must move within determined limits. It is thus ethical rather than psychological in its bent. The author writes with evident enthusiasm and has many valuable things to say. He is perhaps not the first person to say them; he certainly will not be the last.

The Purpose of Education. By St George Lane Fox Pitt. New Cheap Edition, revised and enlarged. Cambridge: At the University Press. 1924. pp. xxix + 92. Price 4*s.* net.

Les Sources et le Développement du Rationalisme dans la Littérature Française de la Renaissance (1533–1601). Par Henri Busson. Paris: Letouzey & Ané. 1922. pp. xvii + 685.

Buddhist Psychology: An Inquiry into the Analysis and Theory of Mind in Pali Literature. (2nd edition.) By Mrs C. A. F. Rhys Davids. London: Luzac & Co. 1924. pp. x + 302. Price (paper covers) 2*s.* 6*d.*, (cloth bound) 4*s.*

Methodos de Psychologia Contemporanea. Pelo Prof. Wacklaw Radecki. Rio de Janeiro. 1924.

Problemas de Psychologia Contemporanea. Pelo Prof. Wacklaw Radecki. Rio de Janeiro. 1924.

What Children should be Told. By G. P. C. Claridge. London: Jarrolds, Ltd. 1924. pp. 20. Price 6*d.* net.

The First Five Thousand. The First Report of the First British Birth Control Clinic. By Dr Marie Stopes. London: John Bale, Sons & Daniellson, Ltd. 1925. pp. 67. 2*s.* 6*d.* net.

Über Begriff und Formen der Intelligenz. Von Otto Lipmann. Sonderabdruck aus der Zeitschrift für angewandte Psychologie. Band 24, Heft 3–4. Leipzig: Johann Ambrosius Barth. S. 48. G.M. 1.50.

NOTES ON RECENT PERIODICALS

Zeitschrift für Psychologie.

Bd 91, Heft 1 und 2, 1922.

Untersuchungen über die Formvisualität. I. (W. Blumenfeld.)

By form visualization the author understands the ability to produce and elaborate two- and three-dimensional form in visual imagery. The purpose of the first part of this investigation is to try to examine more closely a great number of tests in the domain of visual form-imagery, and to report the experiences gathered at this trial with educated adults. It is pointed out that in all professional work where 'quality of execution' counts for anything the time factor is, within limits, of much less importance than the quality of the performance. It appears that, besides the visualization with reference to form, there must be distinguished not only a visualization with respect to colour and another to place or position of a body, but also one with reference to the equally dependent factor of the size of that which is optically given.

Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter. (E. R. Jaensch.)

VI. Übergang zu einer Schichten-analyse des Bewusstseins und einiger seiner Substrate gegründet auf die Strukturanalyse der eidetischen Entwicklungschicht.

Reference is made to the extensive material that has been examined and which, it is alleged, has shown that the eidetic phase is within certain limits a normal stage in development. It is therefore essential to explore this eidetic phase in all directions.

VII. Die Völkerkunde und der eidetische Tatsachenkreis (nebst einer Erörterung über Lévy-Bruhls Psychologie der Naturvölker).

The author raises the questions whether the eidetic disposition, which he alleges he found to obtain so largely among juveniles of the civilized races and which mostly disappears with the advance in years, is less stunted with primitive races. The material collected by Lévy-Bruhl, Jaensch considers, forces upon one the conviction that the perceptual and ideational world of primitives closely resembles the eidetic world of juveniles.

Zeitschrift für Psychologie.

Bd 91, Heft 3–5, 1923.

Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung. Herausgegeben v. F. Schumann.

III. Abhandlung: Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung von Wilhelm Fuchs.

The problem is whether there is in visual space a simultaneous 'one behind another' in the same optical direction; whether in monocular fixation the same retinal part, or with binocular fixation corresponding retinal parts, can give rise to two differently coloured impressions appearing one behind the other. Helmholtz and others answered this question in the affirmative, whilst Hering and others denied it. The transparency and the 'behind one another' were investigated only with plane forms with the unequivocal result that there is a simultaneous 'one behind another' in visual space.

Untersuchungen über die Formvisualität. II. (W. Blumenfeld.)

The object of the investigation was to evolve a new method to test the ability of visual form representation and to put the method itself to the test. Figures made of bent wire were used for the purpose of these experiments. The experiments were as follows: (1) Two like or two unlike figures were given in various positions and it was to be decided whether they were alike or symmetrical (comparison). (2) Such a figure was presented for a definite or an indefinite period in order to be 'learnt' and was then removed. It was then to be decided whether a figure presented subsequently in a different position was the same or was not the same as the learnt figure (recognition). (3) Drawings of one or several such figures in perspective or orthogonal projections were presented and the figure had to be picked out from a group of similar figures. (4) A number of like and similar, respectively symmetrical, figures was to be arranged in groups. (5) A technical drawing had to be made of a figure in a position little suitable for such a drawing. (6) A wire had to be bent according to a technical drawing.

The author comes to the conclusion that with his own method a test of the ability to visualize can be satisfactorily performed and shows several advantages over the previously described methods. The time required to perform such a test is 12–15 minutes.

Zeitschrift für Psychologie.

Bd 91, Heft 6, 1923.

Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter. (E. R. Jaensch.)

VIII. Der Aufbau der räumlichen Wahrnehmungen. (H. Freiling und E. R. Jaensch.)

Eye-movements and movements of the attention generally accompany each other. The determination of each of these constituent components can only be done by experiment. This is performed in the following manner. The subject is seated on a turn-table and obtains an after-image by fixating a coloured square or an 'Anschauungsbild' (memory image) by looking at it without fixation. The subject is then turned round his longitudinal axis, evidently with open eyes, until nystagmus is observed. He then describes the behaviour of the after-image and that of the 'Anschauungsbild' respectively.

IX. Beziehung von Erlebnisanalyse und Sprachwissenschaft, erläutert an den Verben der sinnlichen Wahrnehmung. (E. R. Jaensch.)

This is an attempt to apply the 'eidetic' theory to the elucidation of some philological difficulties.

Untersuchungen über die Frage der sensorischen Asymmetrie. (F. F. Hazelhoff and Heleen Wiersma.)

These investigations were designed to check the work of previous workers on sensory asymmetry.

Zeitschrift für Psychologie.

Bd 92, Heft 1 und 2, 1923.

Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter. (E. R. Jaensch.)

XII. Der Umbau der Wahrnehmungslehre und die Kantischen Weltanschauungen.

This interesting paper forms the twelfth part of the author's series "On the construction of the Perceptual World and its structure in juvenile age." It is philosophical rather than strictly psychological.

Weitere Beiträge zur Sozial- und Individualpsychologie des Haushuhns. (Thorleif Schjelderup-Erbe.)

This is a paper on the psychology of the domestic fowl. The first chapter deals with the various sounds emitted by the birds. The author distinguishes ten of these which are partly different for hens and cocks and describes and discusses them at some length. The second chapter, entitled "Mimicry of fowls," discusses blushing and describes some very interesting experiments showing that the birds recognize each other principally by their appearance. The third chapter deals with the memory of fowls and with further researches respecting the pecking-law.

Experimentelle Untersuchungen zum Problem des Verstehens. (Alice Hermann-Cziner.)

These experiments were concerned with the understanding of meaning of words written or printed on cardboards. The object was not to formulate a new theory of understanding, but rather to accumulate material for such. The method employed was that of systematic experimental self-observation, as the author aimed at pure description.

Zeitschrift für Psychologie.

Bd 92, Heft 3 und 4, 1923.

Starre eidetische Klang- und Schmerzbilder und die eidetische Konstellation. (Hans Henning.)

There are two types of eidetic images, mobile and rigid. This difference, although it may appear paradoxical, also obtains among auditory images. A physiological theory is suggested for both rigid and mobile eidetic images.

Eine neuartige Komplexsynaesthesie und Komplexzuordnung. (Hans Henning.)

This paper describes the case of a student who experiences a curious kind of synaesthesia. "I can in my consciousness associate to each letter and each number a colour. This colour-experience is exactly the same as if I imagine a colour without producing a visual ideational image." It is a colour experience without colour. It consists in the disposition of consciousness or quality of the complex as it is present in objectively produced colour-perceptions.

Ein neuartiger Tiefeneindruck. (Hans Henning.)

Description of arrangement to produce marked plastic effects by means of a 'Rasterdiapositive.'

Über die Asymmetrie der Unterschiedsempfindlichkeit. (Tanenari Chiba.)

An investigation on the asymmetry of difference-discrimination. The senses investigated were touch (intensity of pressure), hearing (intensity of sound and pitch) and sight (intensity of light). The results confirm those of previous observers. It is more difficult to distinguish decrease of pressure than increase. With sound intensities an increase is more easily noticed than a decrease, whilst the discrimination of pitch varied with the source of sound. With light-intensities the results appear to have been somewhat uncertain, but the author thinks that asymmetry may also be assumed here. In short, on the whole asymmetry of difference discrimination may be assumed to exist, although greater or less discrepancies may occur according to the sense investigated and the method of experimentation.

Zeitschrift für Psychologie.

Bd 92, Heft 5 und 6, 1923.

Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten ('Angleichungserscheinungen'). (Wilhelm Fuchs.)

This research has grown out of the same author's work on the simultaneous appearance of 'one behind another.' The protruding parts not only made the structural separation as such possible, but it was also soon noticeable that they had some influence upon the colour of the critical part. This investigation sought to elucidate the following questions. (1) According to what laws do the protruding parts influence the colour of the critical part? (2) Does, inversely, the colour of the critical part also influence the colour of the protruding parts? (3) Is it essential that in order to influence the colour the parts influencing one another should be adjoining, or does the assimilation-effect also occur where there are separating spaces? (4) Does assimilation occur with after-image colours and contrast colours?

Über die 'Art' eines Menschen und das Erlebnis der 'Maske.' (Else Voigtländer.)

This is a characterological sketch on the 'sort,' the 'kernel' of a man and his 'appearance,' to others, his 'mask,' terminating in a discussion on Nietzsche.

Über den Okkultismus. (Karl Marbe.)

A discussion on v. Schrenck-Notzing's book on "Materialisation phenomena" and an exposure of spiritualistic traffic. A repetition of an invitation to spiritualistic mediums to produce their manifestations in Marbe's Institute. Needless to say, no one has availed himself of it.

Psychologische Erfahrungen mit einem Zeitsignal. (J. Plassmann.)

A second communication of the reception of the Paris time signal.

Journal de Psychologie, normale et pathologique.

XXIe Année, Nos. 1–3, January to March, 1924.

A special number dealing with the "Psychology of the Child" and "Pedagogy," with an introduction by Paul Lapie, discussing the question whether a knowledge of psychology is essential, optional or a disadvantage to the teacher, and proving that it is absolutely necessary.

The first few articles deal with the problem of the child's first attempts at speech.

L'Activité linguistique de l'Enfant. (H. Delacroix.)

This article draws attention to the child's acquisition of speech. Adults adopt for its sake baby language and gradually lead it forward to a wider comprehension of words. He has found that children who are neglected by their parents have a limited vocabulary, and begin to talk very much later than others.

La Phrase dans le Langage de l'Enfant. (O. Bloch.)

An account of the development of the child's capacity to form phrases and to understand the relationship of words in a sentence. At first this sense is absent, and isolated words are placed in juxtaposition without any connecting links.

La Mesure du Langage de l'Enfant. (A. Descoeudres.)

An attempt to discover the number of words in the vocabulary of children from two years old to seven respectively, and the relative increase to be observed with each additional year.

Les Traits principaux de la Logique de l'Enfant. (J. Piaget.)

This author remarks that logical activity does not cover the range of intellectual activity and that a person can be intellectual without being logical. The thought of a child is distinctly egocentric. In order to understand the logic of a child, one must first discover to what extent it can communicate with others and tries to conform to the thoughts of another.

Théorie de la Forme et Psychologie de l'Enfant. (Koffka.)

An account of the new theory of 'form,' in relation to child psychology.

Le Problème de la Perception de l'Espèce et la Psychologie de l'Enfant. (P. Guillaume.)

A perception of space is the result of our utilization of other sensory abilities, tactile, visual, auditive. A comparison with memory images of previous experiences is required before our judgment can be influenced by our observation. These are differences which a child can hardly be expected to appreciate and we find by experience that it is a process which develops comparatively late.

L'Asymétrie dans le Développement sensitivo-moteur de l'Enfant. (A. Tournay.)

Observations made upon a child between 4–7 months concerning the use of right and left hands respectively.

Quelques Considérations à propos de l'Intérêt chez l'Enfant. (O. Decroly.)

A careful investigation of the interests of children from early infancy to about the end of the fourteenth year, collected from observations of mothers upon infants, regarding preferences for toys and games, and later mental and bodily activities connected with work done in an elementary school.

Ce que voient les Yeux d'Enfant. (E. Cramaussel.)

An essay upon the question whether the child sees what we see but with less experience and rapidity, or in some totally different manner peculiar to itself, which the adult has abandoned and forgotten and the psychologist cannot reconstruct. The conclusion reached is that seeing is largely a matter of experience and knowledge.

Interrogation chez l'Enfant. (H. Wallon.)

Interrogation is the outward expression of a child's curiosity. At different ages, different subjects attract this curiosity and one finds the questioning expression varying accordingly. Interrogation implies a provisional break in the course and system of our intellectual operations and with abuse may develop into a state of psychic dissociation, doubt and fear. The interrogatory attitude is not one to be encouraged, as an antidote it is well to develop the spirit of invention and personal discovery.

La Narration graphique chez l'Enfant. (G. H. Luquet.)

Notes upon the various types of graphic narration produced by children: (1) The adult or professional, where picture and description are placed side by side, (2) repetitive and (3) fragmentary. Children at an early age reproduce only those actions which interest them most profoundly, other portions of the narrative, equally important, will be left out. Many children's drawings are symbolical, but the present writer does not think this method plays any very great part until after the eleventh year, which seems an astonishingly high age.

Acuités sensorielles et Enfants arrières. (Fouquet.)

A series of careful tests upon sensorial acuity, carried out with backward children and giving statistics of results. The author arrives at the conclusion that the appearance of mental retardation is often given by some deficiency of sensorial acuity.

Enfants vagabonds et Conflits mentaux. (P. Bovet.)

The chief problem in connection with vagabond children appears to this writer to be the reason why they left their parents. The reply, he believes, is to be found in the fact that such children are in a state of mental conflict. To relieve this state two methods may be adopted, the *dream* and the *appropriate action.* The chief cause of this mental conflict in childhood is conflict with parents, leading to fear and rebellion; although jealousy, and the emotions aroused by it often play an important part.

Le Cinéma comme Méthode d'Étude de l'Enfant. (Ed. Claparède.)

M. Claparède believes the cinematograph method to possess a great future where the psychogenetic study of children is concerned, especially when films can be made of their games and activities without their knowledge.

Les Projets de Réforme scolaire à la Fin de l'Ancien Régime. (J. Letaconnoux.)

A discussion of arguments for and against the Jesuit Colleges, which have been suppressed in France, as to whether priests, who have renounced family life, were suitable to be placed as 'fathers' at the head of educational establishments, and whether, their profession having withdrawn them from the affairs of men, they could pass on to others a taste for business and the useful arts.

Journal de Psychologie, normale et pathologique.

XXIe Année, No. 4, 15 April, 1924.

La Science et la Quantité. (E. Meyerson.)

An attempt to compare the scientific or quasi-scientific theories of the past with the forms of science adopted at the present day.

La Dualité de l'Esprit-vie et de l'Esprit-connaissance. Note de Philosophie comparée. (P. Masson.)

It appears that whereas the most primitive forms of civilization attribute the exercise of their vital activity to an original principle, a certain degree of intellectual development is necessary before the speculative rôle of the spirit is realized. This paper gives an analysis of the question.

La Documentation psychiatrique dans 'l'Intelligence' de Taine. (Ch. Blondel.)

A criticism of a communication made at the congress of Philosophy in December, 1921. The present writer points out that Taine was unable to make sufficient use of the documentary evidence of psychiatry, because his clinical experience was too small to allow him to weigh the relative importance of his material or authorities.

Sur les Hallucinations visuelles. (H. Bouyer.)

Notes upon various forms and causes of visual hallucinations due chiefly to (1) toxic infection, (2) unbalanced imagination, (3) too feeble judgment in discriminating objective and subjective stimulation.

Un Procédé d'Enrégistrement objectif de l'Image mentale. Le Réflexe graphique. (G. Dwelshauvers.)

An account of laboratory experiments to determine the existence of non-perceptible movements produced by the fingers, when the subject is presented with the visual image of a body in regular oscillatory movement or an auditory image consisting of a succession of regular beats.

Journal de Psychologie, normale et pathologique.

XXe Année, No. 9, 15 November, 1923.

Religions orientales en Occident. (J. G. Frazer.)

An explanation of the similarities and the differences between eastern and western religions and the influence exercised by the one over the other, in adopting and adapting eastern rituals to suit the temperament of western peoples. The two religions, Christianity and Buddhism, the author explains, have always placed a halo around poverty and celibacy, which saps the main root, not only of civil society but also of the human race, so that, either through wisdom or folly, the majority of mankind have refused to purchase the peace of their souls at the price of the certain extinction of their species.

Linguistique et Psychologie. (H. Delacroix.)

This contribution forms the first chapter of a book upon *Le Langage et la Pensée*, which appeared in March, 1924. We find here a review of other writers' opinions and research respecting the study of speech and language, here treated as two separate ideas. "Language is syntax more than nomenclature," he states, "and the grouping of verbal forms in the mind is a deed of intellectual activity very far from a simple association game."

L'Attitude mentale. (F. Paulhan.)

An investigation and analysis of what is actually meant by the expression, 'attitude of mind,' and all that it may signify, as well as the influence such an attitude can exercise upon thought and action both in the individual and in a race.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

May 16, 1925. (With the Aesthetics Section.)
Intuition and Beauty by Mrs. Ursula Roberts.
The Influence of the Relation between Values and Choice, by Miss H. M. Wells.

SECTIONAL MEETINGS.

Educational Section.

April 6, 1925. Scientific Pedagogy, by Dr Jessie White.

May 11, 1925. Some Considerations on the Cinema in Education, by Miss Barbara Low.

Medical Section.

March 25, 1925. The Conception of Sexuality by Dr J. A. Hadfield, Dr James Glover and Mr A. F. Shand.

April 22, 1925. The 'Synthesis' of an Anxiety-Neurosis, by Dr A. Wohlgemuth.

May 27, 1925. The Relation of the Pleasure-Pain Principle of Freud to the Question of Growth, by Lionel S. Penrose.

EIGHTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Eighth International Congress of Psychology will meet in Holland at Groningen from Sept. 6 till Sept. 11, 1926. The Organising Committee consists of the following:

G. Heymans, *President.*
E. D. Wiersma, *Vice-President.*
F. Roels, *First Secretary.*
H. J. F. W. Brugmans, *Second Secretary.*
L. Bouman.
G. van Wayenburg.
H. Zwaardemaker.

Further information may be obtained from Prof. Dr F. Roels, 86 Maliebaan, Utrecht, Holland.

VOLUME XVI OCTOBER, 1925 PART 2

THE BRITISH
JOURNAL OF PSYCHOLOGY
GENERAL SECTION

MOTIVES IN ACQUIRING SKILL

BY ISABEL BURNETT AND T. H. PEAR.

(*From the Department of Psychology, University of Manchester.*)

I. *Introductory* (pp. 77, 78).
II. *Object of the paper* (pp. 78–80).
III. *Motives in the acquisition of skill in play* (pp. 80–82).
IV. *The acquisition of skill in work* (p. 82).
V. *The function of the teacher* (p. 83).
VI. *Motives which might be used in laboratory investigation* (pp. 84, 85).

I. *Introductory.*

THIS paper[1] may best be introduced by the concluding remarks of F. A. C. Perrin's article "The Experimental Study of Motor Ability[2]."

> A third group of factors (in acquiring motor ability) are those of the emotional and temperamental variety. Inferiority complexes, shifting states of embarrassment and elation, stimulating and inhibiting ideas of unknown origin and character, were only too evident. The psychology of motor accomplishment awaits the formation of a psychology of motivation.

Both Perrin's work, and that of Bernard Muscio[3], show that there seems to be a low correlation between the same person's performance in different *simple* tasks involving muscular control with the minimum of intelligence. The test results do not justify our speaking of motor capacity or motor dexterity, but only of *motor capacities* and *dexterities.* While, as is well known, tests involving intelligence correlate highly amongst themselves, simple motor tests give results which show no significant correlations one with another.

It is not easy to reconstruct the motivation which may be supposed to have lain behind the test-performances in any investigations of the nature just referred to. To a certain extent the motives may have been a desire to please the experimenter, the belief that a good performance was expected of members of the department, and possibly self-respect. But it seems fair to suppose that the subjects would have been less disturbed if they had done the motor tasks badly than if they had made low scores in the ordinary intelligence tests. For if one is a member of a university community, there is an incentive to do one's best at tasks which are alleged to involve intelligence. But no disgrace or negative self-feeling necessarily attaches to a bad performance in card-sorting or in aiming a pencil at a paper target.

---

[1] Read before the British Psychological Society on Feb. 21st, 1925.

[2] *J. of Exp. Psychol.* 1921, pp. 24–56.

[3] "Motor Capacity with special reference to Vocational Guidance," this *Journal*, 1922, XIII, 157–184.

It is possible that the reason for the low correlation found by Perrin and Muscio may be that the acts of skill were controlled by low levels of the nervous system and that the relatively simple muscle groups involved had very little in common with each other. More complicated tasks might have involved the participation of a high degree of motor skill, *i.e.* not a common excellence in all the simple units but a common (a different and higher) power to use them all[1].

There is however another suggestion which we should like to make tentatively: that often the motivation in the acquisition of motor accomplishments or of skill may itself be a cementing force which binds together the simple acts. Generally when we learn skills we desire, and sometimes desire very intensely, to learn them. Such desire may be an agency which is responsible for certain types of skill going together in any one individual. Others may be well organized low-level muscle groups, and possibly even a high degree of kinaesthetic intelligence[2].

II. *Object of the paper.*

The object of this paper is to discuss the part played by motives in the acquisition of skill in work and play, and to consider how motives may be studied in the laboratory.

Motives are those forces impelling us to action which arise primarily from mental or bodily dispositions and only secondarily from the nature of the external situation. This definition covers unconscious motives; it will also allow for the fact that we may not be aware of our motives hough they are on the conscious level, and conversely that we may be mistaken in what appear to be our motives. Motives are conative and have a dual aspect, (*a*) that of being driving forces, impelling us to act in a certain way, and (*b*) that of providing goals towards which the organism strives[1]. In the words of Boyd Barrett, motives are both "efficient and final[2]."

---

[1] Cf. T. H. Pear, *Skill in Work and Play*, London, 1924, pp. 22 f.

[2] Cf. T. H. Pear, *Remembering and Forgetting*, London, 1922, pp. 205–231.

Motives, like the instincts from which they are presumably derived, lie behind every action. From everyday experience we have reason to suspect that the strength of the motive directly affects the individual's performance. We wish here to emphasize the importance of the direct operation of motives upon the acquisition of skilled motor acts.

For example, as has been demonstrated in the laboratory, one of the essentials in the acquisition of any complicated skill is prolonged effort. Bryan and Harter[3] say, "it is the intense effort that educates." Johnson[4] ascribes plateaus in the practice curve to pauses in effort. W. F. Book[5] says, "less effort was actually put into the work at all those stages where little or no improvement was made."

A corollary of prolonged effort is prolonged attention. These two we may regard as essentials in the acquisition of skill.

What, then, are the conditions of prolonged effort and of a continuous and high degree of attention? Both of these may arise and continue either non-voluntarily, or voluntarily.

This is an important distinction between individual methods of acquiring skill. It is also one of the chief distinctions between attitudes of work and play. For in work, voluntary attention is given to the performance in order that a certain result may be achieved, but in play non-voluntary or implicitly voluntary[6] attention is given to a series of movements which are themselves pleasurable to the performer.

In play, then, especially in games which exercise the kinaesthetic and cutaneous organs, the motive for continuing is the pleasure the individual has in the movements he performs. For in many games, and even in some forms of work, muscular skill may express itself through patterns of bodily structure and bodily control closely related to instinctive patterns. The straightforward, almost childish joy that some adults find in games seems thus comprehensible. For such players games are activities motivated by instinct, and require for their continuance neither voluntary attention nor acts of will to prolong the effort. The predominant motive force is their interest in the pleasure which they derive from their kinaesthetic sensations. It seems reasonable to call this *muscular sensuousness*[1]. And just as the other senses have stimulated persons whose enjoyment of them is epicurean, there are epicures of muscular sensations. Such fortunate people, enjoying not merely the sensations themselves but their higher groupings or patterns, are probably to be found among ballet dancers, figure skaters, swimmers, gymnasts and acrobats.

---

[1] Where the presence of instincts as guiding forces has been assumed McDougall's classifications have been used. Further assumptions will be that interest is conative (McDougall's *Outline of Psychology*, p. 274) and that a wish tends to become dynamic.

[2] *Motive Force and Motivation Tracks*, London, 1911.

[3] "Studies in the Physiology and Psychology of the telegraphic language," *Psychol. Rev.* 1897, IV, 27–53.

[4] "Experiments in Motor Education," *Yale Psych. Studies*, 1902, X, 81–92.

[5] *The Psychology of Skill*, Univ. of Montana Publications in Psychology, 1908.

[6] Cf. G. F. Stout, *Groundwork of Psychology*, 52–54.

However, since, especially in modern times, the nature of play has been complicated by social prestige, by considerations of health and even by the prospect of wage earning, the motives in seeking it have become correspondingly complicated. For many professional athletes indeed the motives must be so intermingled that work and play merge into each other. This does not, of course, imply that an amateur on becoming a professional ceases to enjoy muscular sensations and their groupings, but only that in the sophisticated adult mind the single motive of muscular enjoyment cannot often remain uncomplicated by other and more social motives. Yet there must be professional athletes and artists who are peculiarly fortunate in that for them work and play are the same thing. For the activity which they have made their career gives pleasure due to the satisfaction of instinctive tendencies, and this characterizes play. It is however clear that other instincts and motive forces operate in the playing of games, and the following is an attempt to summarize them.

III. *Motives in the acquisition of skill in play.*

Numerous individuals whom we know personally appear to acquire skill in play when impelled by some of the following incentives:

(1) muscular sensuousness;

(2) delight in the feeling of power over oneself, over other people, and over animals and inanimate things;

(3) overtly social considerations; desire for fun[2], friendship, approbation, self-assertion, self-display;

---

[1] Possibly it plays a part in the 'muscular erotism' of which psychoanalysts write. There seems to be no doubt of its importance as a source of pleasure in certain muscular activities of the postural type.

[2] The laughter which accompanies successful achievements in muscular co-ordination affords an interesting psychological problem.

(4) sublimation; chiefly of such insistent and, in their crude form, disapproved social instincts, such as pugnacity, sex and the variegated expressions of fear, self-assertion, etc.;

(5) sheer repression; partly conscious but very largely unconscious. Processes of this kind shade off into

(6) conscious distraction of the mind[1] by intense learning of difficult skills or by the practice of difficult games of skill in order that there may be fewer opportunities to ponder upon unwanted matters. This is side-tracking of a subject rather than repression.

(7) A retreat from reality.

This last function of games appears in a simple form, when one merely changes the objects of one's interest by deserting the harassing work or responsibilities which are regarded as realities, for an afternoon's play which is not so regarded. But on the other hand, probably many people find relief in highly skilled games because the social and physical settings in which they are practised substitute a semi-serious quasi-reality for the actual reality of financial complexities, family worries, etc. And the importance of this play-world is that while its demands are being met they are really real. Any mistakes made by the player will have to be paid for heavily, and, as in some kinds of football, may even involve physical danger so great that he dare not consciously slacken his attention. Such a play-world is very nearly a reality, for though the player could abandon it if he liked, he could not, without incurring unpopularity, retire at any moment he pleased.

One might even speculate whether, amongst the social class known as the "idle rich," joy in skilled games may be due to the fact that these games often form the most real world. In playing such games well, the players temporarily leave a world which, though sometimes real, is sometimes oppressive in its unreality, for one where values are really coercive, rules are kept, and penalties are not evaded. We have practically no knowledge of this class, and what we do possess may have been gained from biassed novels. But people of an intermediate social class, rich but not quite idle, are often condemned by circumstances to fritter away a good deal of their lives. And athletic skill affords them a world in which they can consciously and voluntarily meet reality.

(8) The courting of danger.

Occasionally one of the chief motives in the acquisition of certain types of skill seems to be the deliberate courting of danger. This may actually be true where the skill is used consciously or unconsciously as a method of repression, distraction or side-tracking. But may it not be rather that the average person engaged in such sports is courting bodily thrills, and that those situations which quickly and certainly give him such thrills, and are socially permissible, are usually dangerous?

---

[1] Cf. *Remembering and Forgetting*, 144–149.

The suggestion has been made that the conception of 'distancing[1],' borrowed from the sphere of aesthetics, is applicable here. We may enjoy the thrill of danger when it is psychically distanced but not when it is an actual fact, devoid of an artistic setting. For a similar reason, reminiscences of dangerous incidents are often pleasurable.

May one go further, and also borrow from aesthetics the notion of catharsis; that just as the pity and terror experienced when watching a tragic representation affords a cleansing and spiritualization of the passions, so do the thrills of fear in dangerous games free us from lesser physical timidities?

IV. *The Acquisition of Skill in Work.*

The motives underlying the acquisition of skill in work are often more complex than those discussed above. In work, a higher degree of voluntary attention is generally needed, involving greater restraint of the organism and a more rapid mental fatigue. Whereas play follows the natural instinctive bent of the individual, work generally involves the inhibition of natural tendencies. Even play if persisted in when the individual has become fatigued or indifferent seems to take on the subjective character of work.

The most obvious and frequent motive of work is wage-earning in order to provide for oneself or for dependents. Now, though this seems to provide a direct appeal to the self-preservative and the parental instincts, yet, inasmuch as the individual's interest is not necessarily in the performance of the activity but in the result, so that his enjoyment is relatively remote from his performance, he must exercise voluntary attention and must continue to perform a series of acts which may have no pleasurable feeling-tone. By acts of will he must whip up the organism to prolonged effort. Naturally many measures have been taken to overcome the mental stress that results where an individual's occupation does not in itself give pleasure. Such are the frequent payment of wages, the stimulation of rivalry among the workers, and the institution of overseers and instructors. The part played by the instructor will now be discussed further.

---

[1] E. Bullough, "Psychical Distance as a Factor in Art and an Aesthetic Principle," this *Journal*, 1912, v, 87–118.

V. *The Function of the Teacher.*

It is probably significant that most strikingly good performances involving muscular skill have been learned from a teacher. In such cases, then, the motivation regarded as a whole may be of a kind seldom found in laboratory investigations. And if this be so, perhaps we ought to distinguish carefully between the sources of motivation in various forms of learning, *e.g.* (*a*) learning something by oneself, (*b*) being taught, (*c*) doing something well muscularly, (*d*) doing it well muscularly and thoroughly, understanding it intellectually, and being accredited by society for both performances.

To take one only of these important social factors, the learning from a teacher. It is fairly certain that one gets on faster with a teacher than without one. A good case might even be made out for the belief that, if an individual has good will and a desire to learn, he or she would do better even with a relatively poor teacher than without one, provided, of course, that the teacher taught nothing wrong. For teachers have other, more personal, functions than merely imparting information, which a book can often do as well or better.

When undertaking to learn something from a teacher, the individual usually assents from the very beginning to the whole process of learning, accepts the self-negatory attitude of pupil to teacher, and tends to obey. Long voluntary effort is of course necessary. But if during the learning period there arises a clear choice between making or not making an effort, the original inner acceptance of the working and learning situations, and the habit of obedience will weight the choice on the side of effort. In the most favourable cases the choice will become automatic. This automatic choice obviously economizes volition and minimizes mental fatigue.

In the case of the learner with no teacher, inner assent to the working and learning situation does occur. But it is not reinforced by the personal and often affective relation of pupil to teacher. Nor is it strengthened by any tendency to obey, except that of obedience to the individual's own commands. But obedience to self-imposed commands involves a special self-imposed control. Hence the necessity for acts of will, for inhibition of other natural tendencies and consequently the occurrence of mental fatigue.

VI. *Motives which might be used in laboratory investigation.*

In future laboratory investigations to discover the relation between motor capacities, attempts might be made to introduce motives more powerful than those which have hitherto been used. The following are suggestions for meeting the very real difficulty of introducing adequate motives into laboratory investigations of skill.

As a result of discussions with laboratory workers and others, the suggestion which seems to be most favourable was that children should be chosen as subjects. The chief reason given was that children would probably take the simpler motor tasks[1] more seriously (*i.e.* emotionally and conatively as well as intellectually) than adults, feeling the same pleasure in success, and lack of pleasure in non-success, as in admittedly intellectual tasks. This all-round seriousness, it seems, cannot be relied upon when adults are the subjects. Furthermore, appeals to acquisitiveness by rewards for work done, or to rivalry, are both possible with children, whereas with adults we believe they would be of small value, for the following reasons.

Acquisition can be appealed to successfully when adult subjects, paid at adequate piece-rates, are employed in the laboratory[2]. But for university students, who are the subjects most easily obtained, this is not a sufficient motive. We are also inclined to think that *real* rivalry between members of the same laboratory or the same department can scarcely be stimulated in tests of this kind. And probably no prize would be large enough to appeal with great intensity to everyone.

If such a performance as typing were chosen, one might offer to teach the subjects something of practical value, and this might act as an incentive. But again the fact that one is not guaranteeing to teach by the best method, or indeed by any good method at all, must be reckoned with in this connection.

To sum up, there seems to be a real difficulty in achieving adequate motivation for laboratory tests of motor skill. This difficulty appears to be decreased where children and paid adults are employed. It is in the hope of obtaining some help in the solution of the problems involved that this paper has been written.

---

[1] This is not to assert, or even to assume, that the motives actuating child subjects in such tests are fewer or less 'vivid' than those actuating adults. That they would be fewer seems probable. Yet certain appeals to relatively simple motives succeed more when children are their object.

[2] Cf. I. Burnett, *An Experimental Investigation into Repetitive Work*, Report No. 30 of the Industrial Fatigue Research Board, 1925.

Moreover in this discussion a more general question has been implicitly raised. Can it be assumed, even with intelligence tests of the usual kind, that each test is performed to the fullest extent of the examiner's powers? If not, how far do factors like motivation and interest affect the results obtained?

(*Manuscript received* 4 *April*, 1925.)

NOTE ON THE SAMPLING ERROR OF TETRAD DIFFERENCES

By C. SPEARMAN AND K. HOLZINGER.

THE following paper is supplementary to the one published in this *Journal*, July 1924. A criterion was there given as to whether each of a set of variables is capable of resolution into two independent factors, the one being specific to this ability alone, whilst the other is common to all of them. This criterion consists in the value $F \equiv r_{13}\,r_{24} - r_{23}\,r_{14}$ (called the 'tetrad difference'), whilst r is the ordinary coefficient of correlation (product moment) and the subscripts indicate any four correlated variables. The divisibility into two factors occurs when, and only when, the true values of F for all tetrads that can be formed from the set of variables in question are always zero, or when the obtained values are attributable entirely to fluctuations in sampling. The main problem in the paper was to find the sampling or 'probable' error of the obtained tetrad differences. This was given, as also two much briefer approximations.

Subsequently, these tetrad differences have been much investigated by us, and the approximative formulae have not been found very satisfactory. In the first place, they give in many cases a value appreciably too small. And secondly, they still involve a great deal of labour, as they have to be applied to every tetrad throughout the set. Thus, for n variables there will be $\frac{n(n-1)}{2}$ different correlations and from these may be formed $\frac{1}{2}\,{}^{n}C_2 \times {}^{n-2}C_2$ different tetrads. For instance, in a set of 14 variables investigated, the number of different tetrads came to no less than 3,003.

To obviate this difficulty we now propose a new approximative formula which is very convenient and has been found quite effective. It deals simultaneously with all the tetrad differences derivable from the whole set of correlations. It only requires the average of these and their dispersion (both taken regardless of sign). It may be expressed in the form

$$\text{p.e.}_F = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{p^2 + s^2} \qquad \ldots\ldots(1),$$

where $p \equiv \bar{r}\,(1 - \bar{r})$, $\bar{r}$ being the average of all the correlations,
$s^2 \equiv$ the average squared deviation from $\bar{r}$, and
$N \equiv$ the number of individuals entering into the correlations.

If either $\bar{r}$ or the number of variables is very small, say under ·2 and 6 respectively, another term should be added under the root, giving

$$\text{p.e.}_F = \frac{1{\cdot}349}{\sqrt{N}} \sqrt{p^2 + \left[1 + \frac{12p}{(n+1)(n-2)}\right] s^2} \qquad \ldots\ldots(2).$$

Again, if either $\bar{r}$ or N is very small (say under ·2 and 50 respectively), yet another term may be added instead of, or besides, the last addition. In this way there ensues, eventually,

$$\text{p.e.}_F = \frac{1{\cdot}349}{\sqrt{N}} \sqrt{p^2 + \left[1 + \frac{12p}{(n+1)(n-2)}\right] s^2 + \frac{(1-\bar{r}^2)^4}{2N}} \quad \ldots(3).$$

The present writers, for their part, have almost always found the simple (1) quite adequate.

By way of illustration we may describe an experiment based on 50 cases and comprising an unusually homogeneous group of pupils aged 12 and 13 years. The nine tests used were the components of the Otis Intelligence Scale, the parts being very similar to the American Army Tests. The observed table of correlations may be set forth as follows:

TABLE I.

Test	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*
a	—	·50	·54	·34	·47	·40	·50	·33	·24
b	·50	—	·39	·56	·51	·43	·36	·32	·15
c	·54	·39	—	·49	.52	·27	·27	·26	·27
d	·34	·56	·49	—	·30	·27	·52	·13	·35
e	·47	·51	·52	·30	—	·35	·14	·33	·18
f	·40	·43	·27	·27	·35	—	·38	·40	·19
g	·50	·36	·27	·52	·14	·38	—	·19	·38
h	·33	·32	·26	·13	·33	·40	·19	—	·29
i	·24	·15	·27	·35	·18	·19	·38	·29	—

All the different tetrad differences for this table were then calculated from left to right[1]. Their frequency distribution is shown in Table II.

TABLE II.

Mid value	·18	·15	·12	·09	·06	·03	·00	–·03	–·06	–·09	–·12	–·15	–·18	–·21
Frequency	10	24	48	78	134	92	106	98	64	52	30	8	8	4

The mean is nearly zero, ·0138. The standard deviation from this mean is ·0758, with Sheppard's correction. The standard deviation from zero is ·0771. $\bar{r} = {\cdot}3483$. $s = {\cdot}122$.

Substituting the two latter values in formula (1) gives $\text{p.e.}_F = {\cdot}0492$; formula (2) gives ·0494, and formula (3) ·0513. The actually obtained median deviation from zero = ·0771 × ·6745 = ·0521, which agrees remarkably well with all three theoretical values derived from the respective formulae; so that in this case, at any rate, every one of the abilities can be resolved into two independent factors, the one being always specific and the other throughout common*.

---

[1] That is to say, in each tetrad the product of the right top and left bottom values is deducted from the product of the left top and right bottom values. The reversed procedure would, of course, give the same numerical difference with reversed sign.

APPENDIX.

Proof of the new Approximative Formula.

As shown in the previous paper,

$$N\sigma_F^2 = [r_{13}^2 + r_{14}^2 + r_{23}^2 + r_{24}^2] - 2\,[r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}] + 4r_{12}r_{13}r_{24}r_{34} + \frac{1}{N}\,[(1 - r_{13}^2)^2\,(1 - r_{24}^2)^2 + (1 - r_{23}^2)^2\,(1 - r_{14}^2)^2] \qquad \text{......(4).}$$

For the present approximation we shall neglect the last term in square brackets. Now the mean of all the r^2's for the whole set of variables

$$\equiv \overline{(\bar{r} + d_r)^2} = \bar{r}^2 + s^2 \qquad \text{......(5).}$$

where scoring indicates mean value. Next, the mean of all terms of the form $r_x r_y r_z$

$$\equiv \overline{r_x r_y r_z} = \bar{r}^3 + 3\bar{r}\,\overline{d_x d_y} + \overline{d_x d_y d_z}.$$

But $$\overline{d_x d_y} = r_{dxdy}\,s^2 = -\frac{1}{m-1}\,s^2\dagger,$$

where m is the number of different correlations, and $\overline{d_x d_y d_z}$ = zero for normal distributions. Neglecting this latter,

$$\overline{r_x r_y r_z} = \bar{r}^3 - \frac{3\bar{r}}{m-1}\,s^2 \qquad \text{......(6).}$$

Next, the mean of all of the terms of the form $r_x r_y r_z r_w$

$$\equiv \overline{r_x r_y r_z r_w} = \bar{r}^4 + 6\bar{r}^2\,\overline{d_x d_y} + 4\bar{r}\,\overline{d_x d_y d_z} + \overline{d_x d_y d_z d_w}.$$

Neglecting $\overline{d_x d_y d_z}$ as before, and noting that $\overline{d_x d_y d_z d_w}$ reduces to

$$\frac{3n}{(m-1)\,(m-2)}\,s^4 - \frac{6}{(m-1)\,(m-2)}\,\bar{d}^4$$

which is in general very small, we get

$$\overline{r_x r_y r_z r_w} = \bar{r}^4 - \frac{6\bar{r}^2}{m-1}\,s^2 \qquad \text{......(7).}$$

From equations (4), (5), (6) and (7) there results at once

$$N\sigma_F^2 = 4\bar{r}^2\,(1 - \bar{r})^2 + 4\left[1 + \frac{6\bar{r}\,(1 - \bar{r})}{m-1}\right]s^2, \qquad \text{......(8).}$$

which is readily convertible into (2). For the final addition shown in (3) see the paper of 1924.

---

[*] All these values were obtained by Holzinger in U.S.A. At the same time, just as good concordance was found by Spearman in England.

[†] See Yule's *Introduction to the Theory of Statistics*, ch. XI, § 11.

(*Manuscript received* 17 *July*, 1925.)

CHARACTER AND TEMPERAMENT TESTS[1]

A PRELIMINARY REPORT.

BY MARY COLLINS.

(*From the Psychological Department, University of Edinburgh.*)

1. *Kohs' Ethical Discrimination Test* (pp. 89–91).
2. *The Brotemarkle Comparison Test* (pp. 91, 92).
3. *The Pressey X–O Test* (pp. 92, 93).
4. *The June-Downey Will-Temperament Test* (pp. 93–97).
5. *Criticisms of the Downey Test* (pp. 97–99).

IN a preliminary study of the problem of devising tests of character and temperament, four of the existing tests have been tried, not on any extensive scale, but merely among students known to the examiner, and others of her acquaintance. Such a preliminary investigation has suggested certain modifications which might be advisable and has given a fairly good insight into the respective values of the tests. It has been possible to compare the results obtained from the tests with the known—or, must we say, the partially known—character of the individual.

1. *Kohs' Ethical Discrimination Test.*

The first test used was *Kohs' Ethical Discrimination Test.* This attempts to measure moral judgment by noting how often the child or adult singles out moral reasons for certain actions in preference to reasons of a more general or personal nature. It is not unlike an intelligence test but with moral situations substituted. Arranged in booklet form it comprises six tests, all of them well known except number five—'Offense Evaluation'—which seems to be a new test. A list of 50 'offenses' is given and the subject is asked to evaluate each of them by placing a cross in the appropriate column. The columns are headed Praise, Nothing, Scold, Jail, Prison, Kill. The names of the other five tests indicate their nature; Social Relations, Moral Judgments, Proverbs, Definitions of Moral Terms and Moral Problems. Each test is scored on a percentage basis, and the average score constitutes the moral I.Q. from which the corresponding moral age can be ascertained. The highest score is equivalent to a moral age of 17 years 4 months.

---

[1] The writer desires to acknowledge a grant from the Carnegie Trust for the Universities of Scotand for the purposes of this research.

Any score above 92 indicates the average adult with a moral age of 16 upwards; from 86–91 the sub-average adult, 15 years upwards; below that, ranging from 68–85, are the mildly inadequate, the inadequate, and the strongly inadequate, with moral ages from 12 to 15. From 67 downwards, that is from 11 years 10 months, moral deficiency is indicated. No time limit is imposed to this test when it is applied to an individual, although each test is separately timed. When applied to a group a definite time limit, varying from 1¼ to 4 minutes, is set for each test.

The test has in some respects proved very satisfactory, and may be an attempt in the right direction. It has the considerable advantage of being very easily scored. But whether it actually gives information regarding the moral character or not, is more difficult to judge. It certainly tests intelligence, and those who have a high I.Q. seem to possess also, according to this test, a high moral I.Q. The converse is also true; one boy of 17 years chronologically (mental age 11 years) who had been convicted of theft, came out with a moral I.Q. of 49—a middle grade moral deficient with a moral age of 8 years 10 months. Test No. 4, that of the definition of moral terms, is arranged in the usual way, the words becoming progressively more difficult. It is obvious that the better educated, or the more highly intelligent individual has an unfair advantage here. The test seems to measure the intelligence of an individual as applied to moral discrimination, or moral concepts. The majority of the students tested have come out as sub-average adults, that is, they have scores ranging from 86 to 91. A few selected individuals have scores 92 to 95, the average adult class.

Further, the results seem to point to the fact that the norms are too high, or rather that the differentiation into average adult, sub-average adult, and so on, is not suitably made. To be described as sub-average, and yet have a score of 91, appears somewhat incongruous. And those who were labelled 'mildly inadequate' by the test, also university students, thought their scores of 84 deserved a better fate. Certainly this test grades individuals in intelligence, but as to whether we can claim more from it seems rather doubtful.

The scoring, although easy and objective, is not always just to an individual. No allowance is made for personal opinion. In the 'Offense Evaluation' test, for example, some subjects place no cross in the last column because they do not believe in capital punishment. This causes a lowering of their score for that test and reduces their moral age accordingly. In fact, although the principle on which the scale is drawn up is excellent, better results would be obtained with a total re-adjusting of the columns, and the adoption of some other scheme of classification.

The first and last tests have proved to be rather simple, particularly the last—that of Moral Problems. Even with low moral quotients, scores of 90 and 100 are customary. These tests would decidedly benefit if made more difficult.

Apart from the above criticisms and certain modifications essential to make the test practical for this country, the principle of the test seems to be good, and it is of a serviceable nature either for group or for individual work. The time taken to complete the test appears to range from about 12 mins. in the case of the adult to about 30 mins. in that of the adolescent.

2. *The Brotemarkle Comparison Test.*

The Brotemarkle Comparison Test is a moral test disguised in the form of an intelligence test. It is based on what Brotemarkle calls the ideational content of moral activity. Our moral actions are dependent on some background which determines our moral conduct. For example, the 'Basic moral principle' is represented by the two extremes, Good and Evil. Seven words are given, Bad, Fair, Mean, Kind, Pure, Wicked, Considerate, and these are to be arranged in their 'comparative relation' between the two extremes. There are seven such arrangements to be made. The test is scored by the number of deviations from the norm in each case.

This test is too difficult for children, and is of little value, so far as any preliminary investigation can show, in determining the morality of adults. At the completion of the test by the subject it is difficult to know what conclusions to draw from it, apart from the number of times he has deviated from the normal. Adults appear to range from 10 to 20 deviations, adolescents have a larger deviation in the 30's and 40's. Perhaps this in itself is significant.

The second part of the test with directions and *questionnaire* is even less serviceable, and it is very difficult to see any possible value which may accrue from it. The directions consist in going over the test already completed and picking out as extremes, from the list given, words which seem better fitted to fill these places than the words already selected. Then one is asked to go over each individual word and place over it the word better suited to express the thought which it prompts in the mind. This is a tremendous undertaking, and not at all worth the trouble taken—for what do the results point to when once obtained?

A similar criticism applies to the questions which follow and which the subject is asked to answer 'very frankly.' They are too difficult, or else are couched in unattractive language, for no one spontaneously attempts them. If attempted, they yield unsatisfactory and very meagre results.

3. *The Pressey X–O Test.*

The principle on which the *Pressey X–O test* is based, that of association, is a good one, and undoubtedly it is a very searching test, if the subject honestly works through it. In the first test lists of mixed words are presented to the subject who is required to cancel out all which have unpleasant feeling tone associated and then to draw a circle round that word in each line which is most unpleasant. This gives considerable insight into an individual's make-up, and may indicate the presence of 'complexes.' The words are arranged in a specific order which enables the experimenter to study the subject's reactions. In this test, for example, are words relating to a *fear* complex, a *disgust* complex, a *sex* complex and a *self-feeling* complex. In the case of the latter, we find arranged in different lines such words as Suspicion, Insult, Rival and so on. Similar hidden classification schemes have been arranged in the other tests, the subject being asked to cross out everything he thinks wrong, everything about which he has ever worried, and in the fourth test the word most closely associated with a given word.

The score reveals two tendencies. The total *affectivity* of the individual is indicated by the number of words crossed out; the total *idiosyncrasy* by the total number of deviations from the normal in the encircled words. That the general results are valid cannot be gainsaid. But the value of the test, of course, lies in the actual words marked by the subject. These results I have been able to compare with the knowledge of the individuals tested, confirmed by discussion of the results with them after the test, and undoubtedly the results have shown high correlation. This seems to be the only attempt so far to measure the emotional life of the individual by means of a simple test.

The outstanding disadvantage of this test (apart from the modifications necessary for this country) is the lack of objective control. With untrustworthy subjects or with subjects totally unknown to the experimenter, it is doubtful whether the results can be considered reliable. Again, where forces of repression are strongly at work, it is difficult for the subject to react adequately to the test. One further essential to make the test entirely efficient is that the necessary *rapport* must exist between subject and experimenter, otherwise the subject may hesitate to be perfectly frank. The time taken to complete the test was found to vary considerably with different subjects, ranging from 15 mins. to 41 mins.

4. *The June-Downey Will-Temperament Test.*

This is one of the most suggestive tests yet devised. Professor June-Downey rests on the assumption that there is sufficient psychological justification for broad outlines of character pattern. In her test, by an analysis of the volitional make-up of the individual, the volitional pattern is revealed. This test, based mainly on handwriting, has developed out of a study of graphology, but is not based on any of the assumptions of the graphologist.

The 'Will-Temperament' test, as it is designated, determines the form assumed by character, and judges such traits as adaptability, self-confidence, power of restraint, patience in detailed work, and other important qualities. The test employs handwriting, normal, slow, rapid, disguised, etc. The tests, twelve in all, are given in a specific order. Each is rated on a score of ten, and the final result is drawn in the form of a graph representing the will-profile of the individual. The tests in this investigation were all applied in their individual form, and no use was made of the group form of the test[1].

The actual tests are divided into three groups of four:

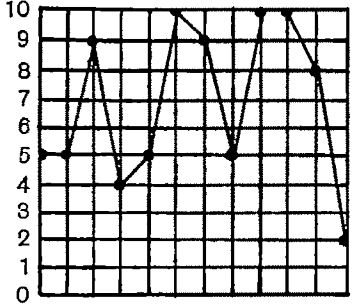
1. Speed of Movement, measured by speed of normal writing.
   Freedom from Load, measured by ratio of normal to speeded writing.
   Flexibility, measured by ability to disguise hand, *plus* success in rapid imitation of handwriting.
   Speed of Decision, measured by time taken to assess own character traits.
2. Motor Impulsion, measured by time and size of writing under distraction.
   Reaction to Contradiction, measured by ability to maintain own opinion in face of contradiction—the 'M.N.' test.
   Resistance to Opposition, measured by reaction to obstacle placed unexpectedly in front of pencil.
   Finality of Judgment, measured by time spent in re-checking assessment of character traits.
3. Motor Inhibition, measured by ability to retard writing.
   Interest in Detail, measured by accuracy of imitation of two models, *plus* difference in time between rapid and slow imitation of the same model.
   Co-ordination of Impulses, measured by ability to write a phrase on a short line in speeded time.
   Volitional Perseveration, measured by total time spent in practising and writing a phrase in disguised hand.

---

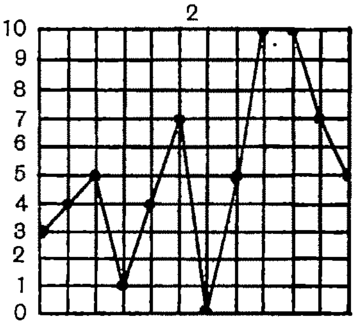
[1] For a full description of the tests, see *The Will-Temperament and its Testing*, New York, 1923.

The profiles obtained have been remarkable in their accuracy, revealing marked characteristics of the individuals they represent. Below are appended six sample profiles with a short description of each.

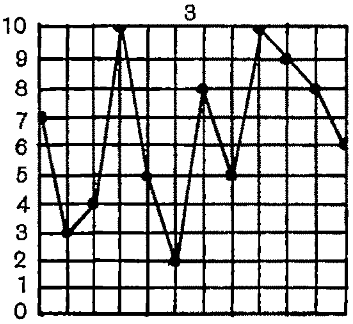
1



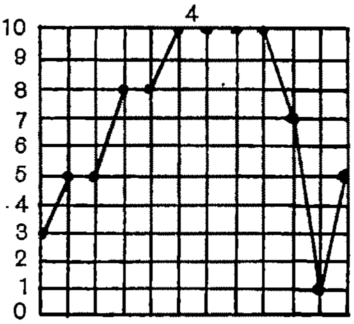
2



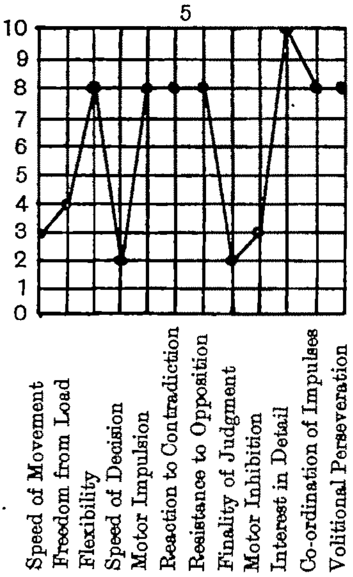
3



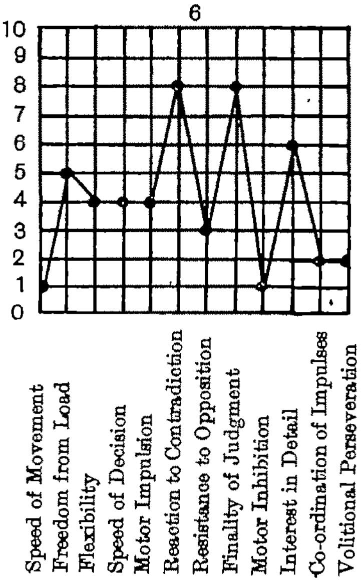
4



5



6



No. 1. The graph runs at a high level throughout. This indicates a strong personality. The total score (82) is a valuable index. A high total score suggests a forceful personality, a low score a weak or colourless individual.

The first four tests represent *speed* items. If these are high, they indicate a quick, flexible reaction.

The second four tests indicate *drive* or personal force or what Downey calls an aggressive reaction. The meaning seems slightly different from the usual one attached to aggressiveness.

The third four indicate *care and persistence,* and suggest a deliberate, methodical, careful reaction.

This profile is high on all three, and gives a three-peaked graph. It represents the profile of a highly successful student. The subject is a girl of strong personality, who has self-confidence and belief in her own abilities. This is confirmed by her assessment of qualities in the character traits where she scores herself in 19 traits out of 22 as 90 per cent. or above. There is a high score in flexibility, which counterbalances to a considerable extent her aggressiveness. She can adapt herself to different situations, is excellent in dramatic work, for example. Moderate speed of decision, at least just below the average, shows carefulness of judgment. Inhibition (scored from retarded writing) and interest in detail also rank high. The only very low score is in 'Volitional Perseveration' and it is difficult to know exactly what this signifies. Downey defines it as 'psychic momentum,' persisting at a task where there is no objectively determined end. This low score, accordingly, suggests a witty and suggestive mind, one which does not think out a problem with all its ramifications. This profile is very similar to Downey's mobile-aggressive pattern, the old choleric temperament.

No. 2. This is the graph of a clever subject. He has strong powers of initiative and is careful and methodical in all he does. He has the slowness requisite for detailed, patient research, but in social relations he has one outstanding temperamental defect which has militated more than once against any chance of success. Notice the low score on 'Resistance to Opposition.' He simply gives up when opposed in any way, not with problems nor with things immaterial, for he ponders these over and over until a solution offers itself. It is only in relation to people, that he cannot fight his way. He is not suggestible, however, as instanced by his high score on 'Resistance to Contradiction.' But this unfortunate trait of poor powers of opposition under opposing circumstances is inimical to success.

No. 3. Here is a graph of a non-aggressive individual, as shown by the low centre scores. In the 'Reaction to Contradiction' test he chose *M* which happens to be the first letter of his surname, but although having a good reason for maintaining *M* to be his choice, he gave in at the second suggestion, that is, on seeing *N* written down on the experimenter's record. Employed in business, he finds this a considerable drawback, for he readily yields to the suggestions of customers, as in fixing prices with them. When the purpose of the test was made known to him, he was rather annoyed with himself for yielding so readily, and was fully conscious of this temperamental disadvantage.

He is strong in traits of perseverance and carefulness, is a plodder and continually trying to improve his abilities. Flexibility is low, which is probably correlated with his shyness. Speed of decision is rather high, which does not seem to correlate with his scores in the last four tests. It suggests inaccuracy in his assessments. It is instructive to note that he describes himself as not suggestible and as possessing self-confidence. The only explanation is that this hasty, inaccurate decision—of course it may be characteristic of him—may be in the nature of a defence mechanism to cover his embarrassment.

No. 4. This profile is strikingly different from its predecessor, and depicts a self-reliant, confident girl of strong personality, one who is accustomed to fight her own battles, as indeed she is. This girl had a stiff upbringing in the school of experience, was left to look after herself at an early age, and in consequence has developed of necessity the qualities essential in such circumstances. The score on speed of decision indicates hasty ill-formed judgment, not the careful, thought-out opinion of the introspectionist. And, when asked to reconsider the marking of the traits, she was impatient over assessing them again, as recorded by her high score in finality of judgment. She evidently is not a waverer. She is not too quick generally, but has tremendous energy and plenty of reserve. The low score on 'Co-ordination of Impulses' is interesting. (This is measured by the success attained in writing the phrase 'United States of America' on a short line of 3 cm. in speeded time.) Just what this meant was explained to her, and she exclaimed, "That's right! If I get too many things to do at once, I get regularly mixed up, and lose my head." It was very striking how this one particular defect came to light. This profile is a very good profile of the individual.

No. 5. This again is a different type of profile and shows a low score on motor inhibition. After three trials of retarded writing, not including the practice trials, this subject found it totally impossible to prolong the writing of 'United States of America' more than 1 min. 55 secs. She tried with all her powers, but found it to be a task quite impossible for her. This seems to suggest a lack of control, a tendency to give up if a task is disagreeable, a failure to achieve a purpose slowly. In social-life, the trait shows itself in lack of reserve. Fortunately, it is combined with interest in detail and persevering powers. It may be of course, if she set herself to the task of retarded writing and practised sufficiently, that a lengthening of time would ultimately show itself, but it does not come easily to her, and would be rather in the nature of an acquired characteristic.

She was much interested in the high score for interest in detail, for a few days previously, her attention had been drawn to this characteristic and it had been suggested that she was over-careful. The central part of the graph reveals a strong personality, and the high score for flexibility suggests a power of adaptability which is certainly correct. She is slow generally or rather deliberate, and reaches decisions after thoughtful consideration; *vide* 'Speed of Decision' and 'Finality of Judgment.'

No. 6. This is the profile of the boy referred to in the discussion of Kohs' test, with the Moral I.Q. of 49. It is generally low throughout, with a total score of 48, which is very significant. It suggests a listless, apathetic individual, lacking in energy. He scores high in 'Reaction to Contradiction' and in 'Finality of Judgment,' but correlated with his other traits they appear to be more of a disadvantage than otherwise. His low score on 'Resistance to Opposition' is significant. He is very slow generally, and only scores 'one' for retarded writing. This indicates extensive lack of control and in fact is one of the problems of his case. 'Interest in Detail' is high, which is the only hopeful sign.

5. *Criticisms of the Downey Test.*

I may briefly indicate certain criticisms in reference to the Downey test:

1. Applied to individuals, the test is time-consuming. It takes approximately 1½ hours to complete the test, and this is a strain on the subject as well as on the examiner. It is the retarded writing that consumes the time. First there is a practice trial, the subject's name being written twice at retarded speed. This practice is not scored. In the real test in which the phrase 'United States of America' is written, a time of 18 mins. is necessary to secure a score of ten. Three trials are allowed. If a subject takes 8 mins. in the first trial, 10 mins. in the

second trial, and succeeds in increasing his time to 18 mins. in the third and last trial, the total time spent on this test will of course unduly burden the test as a whole. It is a relief when the writing is sufficiently retarded at the first trial. When three trials are given Professor Downey scores the third trial, because from a long series of experiments she has concluded that the third trial gives the fairest estimate. In the group form of the test this retarded writing is not so time-consuming, since a time limit is fixed and the score is obtained from the amount done. But in the group test the personal urge of the experimenter is lost, which involves a very significant modification.

2. Too much depends on the personality of the experimenter. The test is not objective from this point of view. In the urge to retarded writing, and more particularly still in the 'Reaction to Contradiction' test, the success or failure of the test in measuring what it attempts to measure rests almost solely with the experimenter. Professor Downey is of course aware of this, and probably in all emotional and volitional tests the allowance for the personal equation of the experimenter will always require to be considerable.

3. The scoring is at times puzzling, and again in some cases rests with the experimenter. Many of the tests are quite objectively scored, but I find, for example, the response to the 'Motor Impulsion' test difficult to place. This is scored in each case by the time *plus* increase or decrease in writing. But where the subject adjusts slowly and breaks down entirely in the middle of the writing, is that to be scored as retarded over 12 mins., or should a second trial be given? Again, where the subject grows confused in counting and simultaneously listening for the tap, tap of the experimenter's pencil, and again breaks down, but manages to carry out instructions in a second trial, is the second trial to be counted? I find that 'Resistance to Opposition' and 'Reaction to Contradiction' present similar difficulties.

4. I should be inclined to change the order of the tests at the beginning. Test 1 is an assessment of character by the subject himself; test 2 is the writing of his name at normal and at speeded rates. Professor Downey suggests that if a subject appears too self-conscious and embarrassed over the first test, we should start with the second, and go back later to the first. This should be the permanent arrangement for all subjects. The writing of the name acts as a 'shock-absorber,' and then the subject is prepared for the assessment of traits, but to present so personal a test first is apt to unbalance a certain type of subject..

5. As regards individual tests: The 'M.N.' test, that is, 'Reaction to Contradiction' is very successful in general, but if the subject has the slightest idea of the object of the test, it entirely loses its value. Substitute tests seem to be essential. The 'Volitional Perseveration' test is not satisfactory. It is scored according to the time involved in writing in a disguised hand, but the instructions are as follows: "Opposite 2 you are to write 'United States of America' disguising your writing so completely that an expert would be unable to identify it. You may try out on the back of this sheet as many disguises as you wish, before entering your choice on the record blank. Take all the time you wish and do your best." Some subjects write the one word 'United' in a disguised hand, are quite pleased with it, and try no more, but begin to write out the phrase in full. The score depends on the length of time taken in practising. Possibly subjects are influenced by the previous speed tests, and undoubtedly the slow worker and slow writer scores higher than the more rapid individual. The test itself seems good; the instructions, on the other hand, require modification.

6. The real character is to a certain extent occasionally masked by practice in handwriting. Those who are accustomed to much writing and speedy writing cover over their real temperament somewhat because of this acquired facility.

7. There is no doubt that the test does reveal outstanding differences among the subjects tested. It differentiates the strong character from the weak, the careful from the careless, and the quick from the slow. It is certainly tapping innate tendencies. There is an extensive field here for further investigation and research.

(*Manuscript received* 1 *April*, 1925.)

SOME FACTORS OF EFFECTIVENESS IN MENTAL ("INTELLIGENCE") TESTS[1]

By S. A. HAMID.

(*From the Psychological Laboratory, University College, London.*)

1. *Introduction* (pp. 100–101).
2. *Procedure* (pp. 101, 102).
3. *Factors* (pp. 102–107):
   A. *Extrinsic.* (*a*) *Position.* (*b*) *Speed.*
   B. *Intrinsic.* (*a*) *Reproduction.* (*b*) *Eduction.*
4. *Difficulties* (pp. 107–113):
   A. *Noetic.* (i) *Fine distinctions.* (ii) *Novelty of process.* (iii) *Complexity:* (*a*) *of fundaments;* (*b*) *of relations.* (iv) *Abstractness.* (v) *Catches.*
   B. *Anoetic* (p. 113).
      Novelty of fundaments or Technicalities.
   C. *Mixed* (p. 113, 114).
      (i) *Equivocal tests and Loose analogies.* (ii) *Very easy tests.*
5. *Conclusion* (pp. 114, 115).

1. INTRODUCTION.

THE following paper proposes to study the part played by the various factors involved in mental tests in giving them their diagnostic value, and possibly to increase their usefulness by discovering the influence of the factors on the test as a whole.

The Data. I am deeply indebted to Professor Spearman, for allowing me the use of records obtained in various official and experimental examinations with a large number of his own General Intelligence tests. These data, as used for this paper, consist of the following:

1. The answers of 90 examinees of the Civil Service Commission, 30 taken from the brightest, 30 from the medium and 30 from the dullest, to 387 tests.

2. The answers of 51 children, divided as above into three equal groups, on the teachers' estimate, to 164 tests.

---

[1] From a thesis approved by the University of London for the degree of Master of Arts.

3. The answers of 88 children, 44 brightest and 44 dullest taken from a much larger number, to an abridged selection of the series used with the last group consisting of 74 tests.

4. The answers of 144 children consisting of 74 bright and 70 dull to 168 tests. In this case to ensure a closer investigation of other factors, the influence of speed was eliminated by giving each test individually and allowing sufficient time for each testee to solve the test or to give it up.

The total number of subjects therefore is 373 and the number of tests studied 793. Nearly 49,000 possible answers to mental tests form the data for this paper.

2. PROCEDURE.

The general procedure adopted for discovering the diagnostic values of tests was very similar to that of Dr C. Brigham in his investigation of the diagnostic value of Binet tests[1]. The total group-scores of the bright and dull testees for each test were compared. In each type, tests that showed the greatest difference in these scores were taken to be the most successful tests in respect of diagnostic value, while those that showed the smallest difference were considered failures. By this method 126 good or successful and 126 bad or unsuccessful tests were selected. These 252 tests were subjected to minute introspective analysis, supported by the suggestive clues contained in the answers of the testees and whenever necessary by the introspections of a trained psychologist.

In the light of such analytical records these tests were all further analysed into their component factors to discover the numerical difference, if any, in the frequency of the factors in the two groups of good and bad tests respectively. The results of this analysis are given under separate headings below.

The quantitative factors were graded into three divisions, *a* (greatest), *b* (medium) and *c* (least). This gradation was made by personal judgment, based on a comparison of a large number of tests. A uniform standard was, as far as possible, maintained in all markings and gradation to avoid the influence of personal equation; but as the work is mainly comparative this influence is reduced to a negligible quantity, being approximately equal in the scores compared.

The Tests. The tests used were of the well-known types usually classed together as 'Intelligence Tests' and are exemplified by the following:

1. Inventive Analogies.
2. Selective Analogies.

---

[1] Psychological Monographs, 24, 1917.

3. The Ebbinghaus completion test.
4. The selective completion test, which differs from the Ebbinghaus test in providing three alternative words for each gap.
5. Giving brief and exact answers to questions relating to a given passage.
6. Opposites.
7. Reconstructing mixed up sentences.
8. Selecting constant attributes of a given object from the alternatives provided.
9. Judging likeliness or otherwsie of a given proposition.

Some slightly modified forms of these tests were also used whenever necessitated by the age of the subjects or found useful as a variation.

3. FACTORS.

A. *Extrinsic.*

To begin with there is a class of factors which may be called extrinsic. These are factors which are not essentially involved in what is intended to be the problem presented to the testee, but instead in the procedure of presentation adopted. Many of these factors may, for the purpose of differentiating individual ability, be safely left out of account. For example all written tests introduce the ordinary operations of reading and writing. The assumption is made, and as a rule justifiably, that such required operations are well within the power of all the testees and therefore exercise no influence on the test as such. All extrinsic factors, however, are not unimportant. A good instance of a significant extrinsic factor in mental tests is the position of the correct answer among the given alternatives in Selective Analogies tests (Table I).

TABLE I. *Distribution of Right and Wrong Answers over the Four Columns of given alternatives in Selective Analogies.*

	1st column				2nd column			
Type of subjects	Max. right	Right answers	Wrong	Total	Max. right	Right answers	Wrong	Total
Bright	120	111	27	138	140	110	36	146
Medium	120	89	97	186	140	67	62	129
Dull	120	35	130	165	140	36	128	164
Total	360	235	254	489	420	213	226	439
	3rd column				4th column			
Bright	160	142	38	180	180	115	7	122
Medium	160	78	50	128	180	74	22	96
Dull	160	37	87	124	180	29	55	84
Total	480	257	175	432	540	218	84	302

(*a*) *Position.* A series of three sets of twenty tests each, applied respectively to three groups of thirty adult subjects consisting of ten bright, ten medium and ten dull subjects, shows that while the bright subjects discover the correct answer wherever it is placed, there is a strong tendency among the dull subjects to mark off the first of the alternatives (if it appears to have the slightest relation to the subject of the test), as if they desire to terminate the emotional tension produced by the attitude of doubt.

In the first column, the maximum number of correct answers is 120. The bright subjects give 111 right and 27 wrong answers, *i.e.* 138 in all; while the dull subjects give 35 right and 130 wrong, *i.e.* 165 answers. The second and third columns show a progressive decrease in the number of answers given, till in the last column the proportion is reversed. Against a maximum number of 180 correct answers the bright subjects give 122 answers while the dull give only 84. Even the bright subjects are not entirely free from this tendency to prefer the first column. A comparison of the total number of their answers with the maximum correct under the first and fourth columns clearly shows this preference. In the grand total under each of the four columns a very strong preference is shown for the first. In the four successive columns of given alternatives 135·83, 104·52, 90·00, and 55·92 per cent. respectively of the maximum number of correct answers falling in each column are given. The difference in the percentages in the first and fourth columns is clearly beyond a chance coincidence.

(*b*) *Speed.* From a comparison of the frequency of occurrence of effective factors, to be dealt with below, in the speed (group 1) and non-speed (group 2) tests, it appears that on an otherwise successful test speed has little influence, if any. The close correspondence of the frequency figures shows that speed does not alter the effectiveness of an otherwise successful test[1]. This is further proved by a practically even distribution of right and wrong answers over all the tests in six sets of ten tests each of Selective Analogies applied to 90 adult subjects (Table II).

B. *Intrinsic.*

Even when the presentation is strictly uniform we find that tests in the same set differ widely in their diagnostic value. One test may show results correlating very highly with the independently estimated intelligence of the subjects while another test of the same form may show

---

[1] pp. 107–114.

TABLE II. *Distribution of Right and Wrong Answers over Tests in order of sequence in the six groups of Selective Analogies.*

No. of Test	Right answers			Wrong answers			Answers omitted		
	Best subjects	Medium subjects	Dull subjects	Best	Medium	Dull	Best	Medium	Dull
1	46	47	14	12	13	43	2	—	3
2	46	44	14	14	16	44	—	—	2
3	45	25	7	14	33	50	1	2	3
4	50	36	17	9	23	40	1	1	3
5	47	38	14	12	20	43	1	2	3
6	48	47	17	10	13	39	2	—	4
7	58	49	14	2	10	42	—	1	4
8	48	34	10	10	25	46	2	1	4
9	46	31	14	13	20	38	1	9	8
10	45	31	13	13	14	39	2	15	8

even negative correlations. For instance the following two Selective Analogies tests show a very remarkable difference.

1. Blacksmith is to Horseshoe as Builder is to ......?
Design, Survey, *Dwelling*, Painting.
2. Duet is to Orchestra-playing as Partners are to ......?
Whist, Company, Business, Finance.

The first gives a difference between the scores of the bright and dull groups of + 30 while the second gives − 8; *i.e.* in the second case the dull subjects actually do better than the bright ones. Since all other conditions are exactly the same, the difference in the effectiveness of the tests evidently depends upon the effectiveness of the tests themselves. In other words the difference is due to factors intrinsic to the tests. Instances of such differences can be indefinitely multiplied.

At the conclusion of his extensive research into the diagnostic value of the Binet-Simon scale, Dr Brigham remarks, "Certain tests are diagnostic of Intelligence (as estimated by teachers) while others are not[1]." Mr Cyril Burt too has found differences in the diagnostic values of the Intelligence tests he has studied and in his book[2] he classifies them according to these values. It is evident therefore that differences in the diagnostic values of tests exist which cannot be explained except on the grounds of intrinsic differences, *i.e.* in the effectiveness of the factors involved and their magnitudes.

We will turn now to factors intrinsic to the problem that constitutes the core of the test. Both the writers mentioned above have classified

[1] Psychological Monographs, 24, 1917, p. 162.
Mental and Scholastic Tests, pp. 143–144.

the tests they have studied on the basis of the factors involved, but what they take as factors are as a rule highly complex groups of mental processes, *e.g.* Ideation, Judgment, Association, Imagination, Critical perception, Reasoning, School-training etc. Summarising his results, Dr Brigham says, "In the writer's opinion the tests that show the highest correlation with intelligence are those which involve reasoning—that demand the application of the subject's knowledge in a new way[1]." This is almost like saying that tests that show the highest correlation with intelligence are those which involve intelligent processes.

The analysis adopted here is based on Professor Spearman's Noegenetic Principles of Cognition[2]. We shall analyse our tests on the basis of these principles into their component factors, consider their influence and that of the various modes of their aggregation, bring evidence to show the value of such influence on the diagnostic value of the tests and cite illustrative examples.

According to the said analysis all cognitive operations consist in more or less complex aggregates of six ultimate elementary processes. They are:

1. Apprehension of Experience.
2. Eduction of Relations.
3. Eduction of Correlates.
4. Reproduction.
5. Clearness Variation.
6. Disparition.

Of these component processes some have comparatively little bearing on the present work. Thus Apprehension of experience is in the main a question of introspection and introspection has so far hardly been brought at all within the scope of mental testing, except to a slight extent by Carey[3]. Nor are we at present concerned with the process of clearness-variation. This is not unimportant in testing. On the contrary it seems to constitute just the point where voluntary control steps in, and moreover the point which affords the largest scope for solving the test by means of diverse procedures. Such scope for diversity is no doubt a very important character in a test. But a proper investigation of this character and its varying degrees would need far more intensive introspective work than has been attempted here. Disparition also need not trouble us. Its nature is essentially negative while tests have essentially positive interest.

---

[1] Psychological Monographs, 24, 1917.

[2] *The Nature of Intelligence and the Principles of Cognition*, pp. 44–108, 137–140.

[3] *British Journal of Psychology*, VII, 1915, pp. 472–480.

As regards the form of individual processes, then, three kinds remain for our chief consideration. These are:

1. Eduction of Relations.
2. Eduction of Correlates.
3. Reproduction.

On the basis of this analysis we have been able to pick out the chief factors or aggregates of factors that contribute towards making a test more or less difficult. Thus we arrive at the list of 'Difficulties' given on p. 100 above (Contents).

The tests analysed have been placed in two groups:—1. Speed-tests comprising 86 most successful and 86 least successful tests applied to the subjects in groups 1, 2 and 3 detailed above (pp. 100–101). 2. Tests comprising 40 best and 40 worst tests in the series applied to subjects in group 4 (p. 101). From group 2 the influence of speed was eliminated, as stated above, by allowing as much time for solution as each subject required.

In the following pages the word 'noetic' is used for brevity's sake instead of the more elaborate though more correct 'noegenetic.'

(*a*) *Reproduction.*

Processes of reproduction, those that recall knowledge previously acquired, play an important part in mental tests. The following two tests are examples of such processes:

1. Money is to Purse as Idea is to ......?
   Thought, Brain, Mind, Feeling.
2. Directory is to Addresses as Atlas is to ......?

Both these require the reproduction of acquired information. They are tests of general knowledge rather than of ability. As instruments of measuring intelligence they are very poor, they do not give fair play to the subject who has not acquired the special piece of information they deal with. They afford no scope for the noegenetic processes and consequently fail to distinguish (according to our present criterion) the intelligent from the unintelligent subject. Their diagnostic values as determined by the ratio between the differences of their group scores and the maximum difference between the scores of the bright and dull groups are respectively 20 and 6·66 per cent.

The frequency of the Reproductive processes is represented by the following:

	Good tests	Bad tests
1st group	5	26
2nd „	6	19

This shows that among 86 tests of high diagnostic value reproduction occupies the chief place only in 5 whereas in 86 unsuccessful tests it occurs 26 times. Similarly in the second group among the 40 good tests it occurs only 6 times, while in the 40 bad tests it occurs 19 times. From this it is easy to see that reproductive processes do not add to the diagnostic value of tests and do not appear to be closely related to intelligence according to our criterion.

(*b*) *Eduction.*

The noetic processes of Relation—or Correlate—Eduction, either singly or combined, occur so rarely in intelligence tests in a state of comparative purity, uninfluenced by their quantitative aspects, that any detailed consideration of them is hardly necessary. The quantitative aspects on the other hand play a very important part and will be dealt with fully, with some other active factors, under the heading of 'Difficulties' below.

4. DIFFICULTIES.

A. *Noetic.*

(i) *Fine Distinctions.*

The fineness of distinction required by a test involves the successive eduction of relations between correlates, of relations between relations and so on in higher orders, before the required correlate can be finally educed. Take for example the following test.

Train is to Station as Ship is to ......?

Port, Sea, Shelter, Destination.

First a relation has to be educed between 'Train' and 'Station.' Secondly a series of relations has to be educed between 'Ship' and the four given alternatives. Thirdly by a trial and error method a process of correlate-eduction has to be performed between the first relation and the second series treated as fundaments, satisfying the relation of similarity. It is here that the actual distinction has to be made, because whereas the relation of 'Ship to Port' exactly satisfies the relation of similarity, the other three relations, those of 'Ship to Sea,' 'Ship to Shelter' etc. possess a semblance of similarity to the first, and are apt to lead the subject astray, if he fails to acquire the requisite degree of clearness of apprehension. He is very likely to select the more closely associated 'Sea' for his answer.

Fineness of distinction shows the following frequency in the tests under analysis:

	1st group		2nd group	
	Good tests	Bad tests	Good tests	Bad tests
a[1]	26	59	5	24
b	33	4	29	4
c	7	3	6	12

These results show:

1. Very fine distinctions prejudice the success of a test. Take, for example, a test such as:

Loss is to Sorrow as Gain is to ......?

Profit, Advantage, Mirth, Enjoyment.

This demands that besides the succession of eductive processes the testee should make distinctions of a fine and involved character between the implications of the concepts of *mirth* and enjoyment, so that even a person of ability well above the average will not readily arrive at a correct solution.

2. A medium degree of fineness of distinction adds to the diagnostic value of intelligence tests.

(ii) *Novelty.*

Novelty may consist in unfamiliarity either (1) of the data presented, *i.e.* fundaments and relations, or (2) of the processes to be performed upon familiar data. In the first case the items lie outside the knowledge of the subject. This type of factor we call technical and shall deal with later. Novelty of process, on the other hand, consists in utilising familiar data in an insightful and originative manner to produce new mental contents. This, of course, is true of all eduction whether of relations or of correlates. But even in eduction degrees of novelty exist, depending on how far a person has treated the terms presented to him in the special way required by the test before. This is not perhaps what most people would be inclined to call novelty of *process*, but since, as would appear on reflection, no other *novel* cognitive production than eduction is possible, this is the only sense in which the term can have any meaning. Difference of degree of novelty may be illustrated by the following two tests:

---

[1] In this and the succeeding tables of frequency of quantitative factors: *a* indicates tests requiring or offering the greatest degree; *b*, tests requiring or offering a moderate degree, and *c*, tests requiring or offering the smallest degree of the factor under consideration (see p. 101).

1. Grass is to Green as Snow is to ......?
Yellow, *White*, Mountain, Black.
2. Couch is to Chair as Luxury is to ......?
Comfort, Wealth, Stool, Pleasure.

In the first test both the fundaments as well as the relations are quite familiar. But in the second though all the items are concepts of everyday use, couch and chair are hardly ever considered in the light of the distinction that obtains between luxury and comfort. The process demanded is decidedly novel.

The frequency of Novelty of Process in the tests under analysis is:

	1st group		2nd group	
	Good tests	Bad tests	Good tests	Bad tests
a	1	6	3	14
b	31	9	24	10
c	5	1	13	16

This shows that:

1. A high degree of novelty does not conduce to the diagnostic value of a test. The following test is an example of this type:

What must a deaf man have in order to understand people at all ......?
Arms, Patience, Sight.

In this test all the items are of a familiar type and yet on account of the novelty of processes involved it is a failure.

2. A moderate degree of novelty adds to the diagnostic value of a test.

(iii) *Complexity.*

Complexity in an intelligence test may be (1) formal or (2) material. Material complexity is a matter of the subject's span of attention. It is also directly concerned with the definiteness of a test. We shall refer to this aspect of it when dealing with equivocal tests. Complexity of formal factors taxes the power and clearness of apprehension. Before solving a test a subject must be able to understand what he has to do and must appreciate the interrelationship of the various parts of the test, one to another. A certain degree of formal complexity is essential for a good mental test. Take for example the following two tests:

1. Soldier is to Weapon as Workman is to ......?
2. The sun was ...... rapidly and ...... all the sky with light.

Each of these tests is the best of its type and is illustrative of the extent of formal complexity that makes a test successful.

The completion test as a type is representative of complexity, both formal and material. All the words going before and after a gap form two complex parts of a compound fundament, while the meaning of the whole sentence forms the other fundament and the omitted word is educed as a relation between the two. Tests in which much complexity has been introduced seem to defy the subject's power of apprehension and clarification especially under examination conditions.

(*a*) *Complexity of fundaments of relations*. One relation may have a number of fundaments interrelated in a complicated manner which require a very high degree of clearness of apprehension before the test can be solved. The following example will help to illustrate this:

> Some people seem to find ...... even in achieving small things, others ...... in great ones.

When we analyse the highly complex processes of the solution of this test we find that they consist of a number of stages. The first consists in understanding the meaning of the entire sentence; the second in dividing the sentence into the three principal parts, viz.:

(1) Some people (fundament)
(2) seem to find ...... (relation)
(3) even in achieving small things, others ...... in great ones (fundament).

Then in the compound relation, the word 'pleasure' is educed as a correlate with 'find' as relation and the preceding words as fundament, to fill in the first gap. Similarly with the second gap.

The degree of complexity is assessed, as all other quantitative factors, by personal judgment based on a comparison of a large number of tests, and on analysis would seem to consist in (A) a multitude of fundaments entering into a relation, (B) relations between relations and so on in higher order and (C) multitude of processes not intimately interrelated. Complexity of fundaments occurs with the following frequency:

	1st group		2nd group	
	Good tests	Bad tests	Good tests	Bad tests
a	8	13	2	14
b	31	21	29	9
c	0	3	9	17

(*b*) *Complexity of relations of same fundament.* Complexity of relations is hardly ever free from abstractness. Its nature will become clear by illustration. In the following Completion Test:

I cannot expect you to ...... my story unless I give you ...... of its truth.

The relations are very complex. Besides the complicated scheme of grammatical interrelations which must govern the whole performance, the meaning of the given sentence forms one of the principal fundaments with which the various parts stand in relations varying according to the nature of the respective parts. The eduction of the first correlated word 'believe' is governed by the relations of expectancy and of truth. The eduction of the appropriate word to fill in the second gap is in turn governed by the relations of expectancy, belief and truth among other minor ones.

In assessing the grades of complexity of relations the order of importance of the constituents is changed from (A)—(B)—(C) (as in Complexity of fundaments, p. 110) to (B)—(A)—(C). Its frequency is represented by the following:

	1st group		2nd group	
	Good tests	Bad tests	Good tests	Bad tests
a	22	40	6	11
b	20	11	27	9
c	3	1	7	20

These show that:

1. An excessive degree of Complexity of relations is not beneficial to the success of a test.

2. A moderate complexity of relations increases the diagnostic value of a test.

(iv) *Abstractness.*

To illustrate what is meant by abstractness, three tests progressively abstract in their material, *i.e.* in the relations and fundaments involved, are given below. From these it will be easy to see what is indicated by the three grades of abstractness (*c*, *b*, *a*, respectively) in the numerical results.

1. Man is to Skin as Tree is to......?

Branch, Leaves, Root, *Bark*.

2. Signpost is to Road as Compass is to ......?

Needle, *Direction*, Ship, Card.

3. Money is to Finance as Labour is to ......?

Wages, Workman, *Industry*, Factory.

The frequency of the various degrees of abstractness in our tests is as follows:

	1st group		2nd group	
	Good tests	Bad tests	Good tests	Bad tests
a	12	40	2	10
b	28	9	28	11
c	11	2	10	19

This shows:

1. A high degree of Abstractness is prejudicial to the diagnostic success of a test.

2. A moderate degree of Abstractness is a factor of great utility in giving tests their diagnostic value.

Abstractness is one of the factors by suitable adjustment of which tests of progressively graded difficulty can be constructed.

(v) *Catches.*

All popular riddles, puzzles, charades and conundrums belong to a larger group of tests of ability. For their solution often no special knowledge is required, but there must be a general aptitude for using familiar data in an insightful and originative manner, for discerning subtle analogies and contrasts. A mental test with a catch however is different from the ordinary riddle. In the riddle the solution usually lies along some by-path of sophistry, while in a test it lies along the straight line of logical reasoning. Let us consider, for instance, the following test:

Train is to Station as Ship is to ......?

Port, Sea, Shelter, Destination.

It will be noticed that in this test the solution does not depend upon any remote and far-fetched mock analogies, but on the discernment of real and distinct relations. What is needed to solve the test is a clear apprehension of the exact relation between a train and a station as distinct from the terminus, from the road and from the end of a particular journey. If a subject does not attain the requisite clearness of apprehension, for him the word 'Sea' which in every-day use is more often associated with 'Ship' than 'Port' offers a very likely pitfall. Let a subject's span of attention be limited or let him relax his vigilance and he is bound to fall into the pit. Such an associational pitfall is what we call a 'Catch.'

Catches show the following frequency in our tests:

	Good tests	Bad tests
1st group	21	0
2nd „	23	0

This shows that 'catches' are a factor of great utility in devising mental tests and are conducive to their success.

B. *Anoetic.*

Novelty of Fundaments or Technicalities. By these we mean items pertaining to special spheres of knowledge that one does not acquire in the ordinary course of life, although on their surface the tests containing them do not appear to do this. Two examples will help to illustrate the type.

1. House is to Windows as Mind is to ......?

Brain, Thinking, Body, *Senses.*

2. Frank is to Open as Underhanded is to ......?

Dishonest, Deceitful, Deception, Frankness.

In the first test the concept of 'Senses' as an avenue of mental illumination is peculiar to Psychology. 'Senses' to the layman means wits, or reason or the logical consistency of behaviour. Consequently the answers given by all the testees bright and dull alike were brain or thinking or body.

The second test makes the distinction between the alternatives too fine and purely grammatical with the result that all the alternatives are equally favoured.

Novelty of fundaments or technicalities prejudice the diagnostic value of a test for they often convey too little to the subject's mind to enable him to educe any relations between them. This is corroborated by their frequency results which are as follows:

	Good tests	Bad tests
1st group	3	21
2nd „	2	19

C. *Mixed.*

(i) *Equivocal Tests and Loose Analogies.* These are tests in which one or more of the fundaments admit of two or more meanings and consequently a number of terms can be found which answer the test equally well. The chief defect in these tests is that a testee might take up any one of the points of view possible and arrive at a solution which so far as consistent thinking is concerned may be perfectly sound, but has to be discredited on methodological grounds. And this applies equally to Loose Analogies involved in tests. The following two tests illustrate these characters:

1. Duet is to Orchestra-playing as Partners are to ......?
Whist, Company, Business, Finance.

In the above test the term 'Partners' is equally applicable to 'Whist,' 'Company' or 'Business.' Again the relation obtaining between 'Duet' and 'Orchestra' is not exactly analogous to that obtaining between 'Partners' and the correct answer 'Whist.' The analogy is very indefinite and consequently the test is a failure.

2. Stick is to Blind as Signs are to ......?
Fingers, Speaking, *Dumb*, Language.

In this test the equivocation is of a very subtle and therefore embarrassing kind. Blind may be taken as a verb as readily as a noun. Again a stick is an objective means of guidance to the blind but a dumb person makes signs but does not require signs made to him. The analogy therefore fails. These two characters in combination show the following frequency in the first group of tests here analysed.

Good tests	Bad tests
2	26

(ii) *Very Easy Tests.*

Such tests may be employed for purposes of illustration but for actual testing have no value at all. They only cause fatigue and increase boredom. It is besides methodologically futile to apply a test such that every testee can solve. The following is an example of such a test:

Ounce is to Pound as Inch is to ......?

Very easy tests show the following frequency among those analysed here.

	Good tests	Bad tests
1st group	0	3
2nd „	0	17

5. CONCLUSION.

In the foregoing pages we have attempted to show the part played by certain component factors of mental tests in determining their diagnostic value and have tried to show the influence that varying extents of these factors have on the success of tests. Two conclusions stand out clearly as the most important guides in test-construction. First, that a medium degree of difficulty, a degree that does not carry the solution beyond the apprehension of the subject, is the most important factor in the success of a test. Very easy tests are just as useless as very difficult ones. Secondly, that Eduction and not Reproduction is the only reliable basis of a successful test.

This gives us sure ground to stand upon. The results of this study offer rules based on positive quantitative evidence for the construction of Intelligence tests. For our analysis of tests we have adopted the criterion (the only one available for determining the value of tests) of the independent estimates of teachers, and tests constructed according to the rules based upon these results will at least satisfy that criterion.

Hitherto for want of any such rules the construction of Intelligence tests has been attempted in a haphazard manner. Tests have been constructed according to the personal opinion of each author, but no one could predict their diagnostic value. This entailed an unnecessary waste of much time and energy. Our rules provide a safeguard against such waste. They appear to reduce test-construction to a more scientific and determinate process.

These rules by their very nature throw some light on the problem of the nature of Intelligence as estimated by teachers. They supply some definite indications as to what Intelligence tests actually measure. By indicating the best means of measuring an entity they in some measure define the nature of the entity measured.

From the nature of the teacher's estimate based as it must be on school-work it might perhaps have been expected that mental tests would measure predominantly the amount of instruction received, the amount of information acquired. But this is just what they do not do. Tests of information have been shown to have very little diagnostic value. In their case the differences in performance are very slight.

Intelligence tests, again, do not show themselves to measure adaptation to a novel 'situation.' For if that term is to have any meaning at all in reference to tests it must *include* the dealing with unfamiliar words, ideas and facts. And these latter have been shown in most instances to render a test relatively useless.

On the other hand the tests do appear to measure the power to treat familiar data in an insightful and originative manner when these terms are taken in the sense of 'noegenesis.' They measure the 'Noetic' and the 'Generative' powers of the subject in so far as these characterise the eduction of relations and correlates.

(*Manuscript received* 16 *December*, 1924.)

THE COEXISTENCE AND LOCALIZATION OF FEELING.

A REJOINDER.

BY A. WOHLGEMUTH.

1. *The argument* (pp. 116, 117).
2. *'Meaning of feeling' and affective experience* (pp. 117–120).
2. *Coexisting of localized feelings* (pp. 120, 121).
4. *Experimental results of pre-conceived theory* (pp. 121, 122).

1. *The Argument.*

UNDER the same title as the above Dr P. T. Young published in this *Journal* a criticism of my work "Pleasure-Unpleasure: An experimental investigation of the feeling-elements[1]." Among other results I established the following two:

(1) Two or more feeling-elements may coexist in consciousness; they may be like or they may be unlike.

(2) Feeling-elements can often be localized.

Young, engaged on similar investigations[2], came to exactly the opposite conclusions, namely:

(1) Pleasantness and Unpleasantness are not felt at the same time.

(2) Pleasantness and Unpleasantness are not localizable.

We have, then, two pieces of research on the same lines with contradictory results. This fact in itself furnishes a very interesting psychological problem to which we shall return later.

If I understand Young correctly the argument of his criticism may be stated concisely as follows:

If a 'feeling' is referred to an object we are really not dealing with a 'feeling,' but with the '*meaning of a feeling.*'

My instructions to my observers were of such a nature as to produce an attitude favourable to the referring of feelings to an object; and the protocols show that the feelings were so referred. Hence, it follows that the statements about 'feelings' in the protocols are not about feelings really, but are statements about the '*meanings of feelings*,' and the conclusions drawn by me about feelings are therefore unwarranted.

---

[1] *Brit. J. of Psychol.* Monogr. Suppl. No. vi, 1919.

[2] P. T. Young, "An Experimental Study of Mixed Feelings," *Amer. J. of Psychol.* 1918, xxix, 237–271, and "The Localization of Feeling," *ibid.* pp. 420–430.

2. '*Meaning of feeling*' *and affective experience.*

First of all, what is this 'meaning of a feeling,' this 'meaning of pleasantness,' etc.? The possibility of its being a *conative* process does not come into the question at all. It cannot possibly be an *affective* process for then it would be a feeling of pleasure or of unpleasure, for there are no other feelings, and memory-images of feelings do not exist as Külpe has shown conclusively. It can therefore only be a *cognitive* process, that is, *knowledge* of or about feeling. One of my subjects remembered having on a certain occasion heard a song which gave him great pleasure; he remembered the title of the song, but he had not even the faintest auditory image of the melody; yet he remembered, as stated, that he experienced pleasure. This was a piece of knowledge—a cognitive process—concerning the past experience of a feeling. Precisely the same, I find, is also the case whenever a sensory experience is so far forgotten as to leave me unable to reproduce a memory-image of it, although the knowledge that that experience has taken place is there. But pleasure, or unpleasure, must have been experienced before this fact can become a piece of knowledge—a cognitive process. However, when I dictate a protocol I do not record what is then actually taking place at the time of recording, but I give a picture of what, according to my mental notes made during the experiment, happened during the period of introspection of the experiment. This is the usual procedure. Now if we allow, for argument's sake, that an observer records his experience of the experiment in the form of judgments and objective references he is, of course, at the time of recording, dealing with '*meaning* of pleasure-unpleasure.' But in order to have obtained that knowledge he must have had the affective experience, must have *felt* the feeling during the introspective period. If he is a trained and careful observer he *knows* whether two feelings were felt simultaneously or whether a feeling whilst it was being felt was localized, however he may describe his experience afterwards, whether in forms resembling judgments or as objective references. But I go still further than this. I say deliberately that it makes no difference whatever to the coexistence of two feeling-tones, or to the localization of a feeling-tone, whether I designedly and with emphasis refer the feelings to the objects or stimuli, or whether I contemplate my experience as subjectively as possible. I will cite an instance, not recorded after the introspection, but recorded whilst the experience was in progress, whilst the feelings were felt.

Looking out of my study window upon the rockery in front covered with all kinds of spring flowers I fix my gaze upon a clump of lovely daffodils, I experience a great pleasure. I know the pleasure is mine; I experience it, but I experience also the daffodils as beautiful. The combination of their canary-yellow and bright green is so pleasant; it is helped by associations, swift and fleeting, of spring, of the songs of birds, of blue sky, of golden sunshine, of youth, and so on. Whilst thus enjoying the daffodils I have unpleasant perceptions of something in my left foot which is due to a nerve injury. There is a sort of 'wooden, tightening feeling,' with pricking like that of pins and needles, and spasmodic painful contractions of some muscles, the whole experience highly unpleasant. But I hold the pleasant daffodils and the unpleasant foot-sensations simultaneously in consciousness. From this, I think, I may logically conclude that pleasure and unpleasure coexist. Again: I give myself up entirely to my experience, especially to its affective side, neglecting, as far as possible, the cognitive one. I almost forget, at moments, that I am looking at daffodils; I live, I experience, pleasure. When there is no painful muscle contraction in my foot—which generally spoils the experiment—but only the stocking-tightness and tingling sensations, I experience unpleasure. Unpleasure is felt whilst pleasure also is felt, both now, this moment, together. I experience both, pleasure and unpleasure, simultaneously; they are both present now—there is no oscillation—they are there, steady, together, and at the same time. Can I localize them? Yes. The unpleasure is there in my foot, where I experience the tingling and tightness, it is an unpleasant tightness and tingling. But I cannot project the pleasure out on to the daffodils, try as hard as I may. The pleasure appears to reside with faint, but distinct organic sensations in the thorax and arm-pits which it seems to permeate. But though I do localize the experience in this way, I simply live my experience, I refer to no objects: pleasure-unpleasure I experience now, at this moment, together; pleasure there—thorax; unpleasure there—down below—foot. This is for me, who lives this, an indisputable, irrefutable, apodeictic psychic fact. I would as soon doubt my very existence, as this experience.

To contend that the difference of results in these two investigations, Young's and mine, is due to the alleged fact that Young's observers reported about *'feelings felt'* and mine about *'meanings of feelings'* is merely logomachy. My observers, of course, did nothing of the sort: they reported about their experiences, the feelings felt during the introspective period, *i.e.* during the presentation of the stimulus, whether they had felt two feelings simultaneously, or whether they had localized a feeling whilst it was being felt.

During protocolling two attitudes are conceivably possible with respect to the knowledge acquired during introspection. The one, which may relatively be called the subjective attitude where the observer records his experience as he actually lived it; the other, which may relatively be called the objective attitude where he records the knowledge obtained as such. But in whichever attitude the recording is made, it deals in each case with 'feelings felt.' Frequently, if not generally, an observer lapses from one attitude into the other. Whether it be desirable that the observer should rigidly stick to one attitude during recording is a moot point. Personally I hold, and I hold this view strongly, that an insistence upon this interferes with the free and easy recording, and may even impede the introspection, whilst no advantage is gained. A perusal of the full protocol will give an idea of the reliability of the observer than which there is none better. Such protocols in full are available in my investigation, whilst Young contents himself with extracts, although the research was carried out in the laboratory of Cornell University, whence Titchener fulminated against this practice[1].

From the foregoing remarks my views about the 'stimulus error,' the 'meaning error,' the 'meaning of feeling' etc. are obvious. Although I do not wish to say that such errors are non-existent, their importance is in my opinion certainly much exaggerated, and a trained observer is not likely to fall into them during introspection, even if, during subsequent protocolling, he should at times adopt a somewhat objective attitude. It is, therefore, hardly necessary to go through the details of Young's criticism, for that would simply mean repeating myself. Suffice it to say, that I cannot share his view that my instructions to my observers favoured the adoption of an attitude of attending to the stimulus or to objects instead of the experience itself. 'Introspect closely,' 'describe your *experience*,' 'attend to the affective side of your *experience*,' 'attend to these experiences,' 'live, give yourself up to, the whole experience' is enjoined upon the observer over and over again.

The obsession of the 'meaning error' culminates in the following statement by Young[2]: "If a subject reports 'the object was pleasant,' we, of course, cannot doubt that the *meaning of pleasantness* attaches to the object, but we have no guarantee that pleasantness was *felt*." How can the *meaning of pleasantness* be attached to the object, unless the perception of the object had previously given rise to the experience of pleasure? How can this *knowledge* be acquired without the occurrence of the *affective* experience?

---

[1] E. B. Titchener, *Lectures on Elementary Psychology of Feeling and Attention*, New York, 1908, pp. 48 *sqq.*

[2] This *Journal*, xv. 359.

3. *Coexisiting and localized feelings.*

Speaking of the '*Interpretation of mixed feelings*,' Young says[1]: "That so-called mixed feelings depend upon an attitude of observation is shown by the following evidence—(1) there is a well-marked tendency for mixed feelings to be reported in groups of successive reports. If the reader will plot by number the introspections in which coexisting feelings are reported[2], the grouping will be clear." Although it would hardly appear necessary after what I have said above about Young's criticism to go at all into the point raised here, I have nevertheless done so, and the outcome may prove helpful in elucidating the interesting psychological problem of two workers, Young and myself, engaged upon the same investigation arriving at diametrically opposed results. I have gone to the trouble of examining Young's statement with regard to the first of my observers, W., and I find that the experiments designed to give facilities for experiencing coexistence of feelings, namely experiments of class A, groups 3 or 4, partly also group 2, began on October 28th, 1915 and went on till March 3rd, 1916. During this period there were 21 experimental days and, with the exception of December 12th, 1915, on EVERY day there were records of coexistence. On the excepted day only a single experiment of that class was performed. Where then is the grouping upon which Young bases his criticism? Probably, with his preconceived idea, Young turned up pp. 161–2 of my monograph, saw the experimental numbers densest from 60 to 120 and without enquiring any further jumped at once to unwarranted and unverifiable conclusions.

Young continues: "The data show that there is a statistical association between coexisting and localized feelings. When a subject reports coexisting feelings he is apt also to report localized feelings. This implies that one and the same attitude of observation is involved in both cases." To begin with, the conclusion is, to say the least, illogical. But that does not matter. Next let us look at the premises and see whether these, at least, are correct. Taking subject W. we find 29 experiments where both coexistence and localization are reported; 25 experiments where coexistence alone, and 12 where localization alone is reported: that is 29 cases where Young's 'attitude' theory would apply against 37 cases where it does not apply! Or, still better, take subject Z.: here we have 17 experiments where both localization and coexistence were reported, 7 experiments where coexistence only, and 26 where localization only was reported, that is, 17 cases in agreement with Young's theory and 33 cases not in agreement! Further, there are numerous cases, over 20, where at least two feelings were experienced during the experiment, one of which was localized and the other could not be localized! (They are indicated by the sign ± on p. 163 of the monograph.) More stultifying evidence against his theory than that which Young adduces in proof of it can hardly be imagined[1].

---

[1] This *Journal*, xv, 360.

[2] Reference is given to my monograph, pp. 161–3.

4. *Experimental results and pre-conceived theory.*

There remains now only the interesting psychological problem, above referred to, which is raised by Young's investigation and by his criticism of my work. I can, of course, in this paper, merely indicate the direction in which the solution, in my opinion, may be looked for, namely the 'Laboratory Atmosphere' in which Young worked. Titchener's views upon the subject of coexistence have been known for nearly twenty years. Further, in my view, the meaning of the 'meaning of feeling' has been exaggerated to make it appear significant where it has but slight importance, and observers might possibly be more concerned, so it seems to me, with the orthodoxy of their attitude than with the analysis of the psychic experience itself. In fact, I should not be surprised if they judged the correctness of their attitude by their results.

No such 'atmosphere' obtained in University College. No work in this direction had been done there before and the views of workers were divergent and generally founded upon casual self-observation. In this connection it is interesting to refer to the 'statements' made by each of my observers before the experiments were begun and which statements precede the protocols[2]. It will be found here that whilst observers

---

[1] It is fair to Dr Young to state that in the original draft of his paper both of the points dealt with in the last two paragraphs were discussed at considerable length, and the evidence upon which they were based set forth fully. The paper as it finally appeared was condensed at my request, and certain tables which were held to support these points were omitted. This condensation was desired solely in the interests of space. EDITOR.

[2] *Loc. cit.* pp. 22, 73, 106 and 136.

W. and X. had no settled views, observer Y. decidedly inclined towards the view that there could be no coexistence of feelings, and observer Z., a pupil of Wundt's, held the same view in a pronounced manner. All observers came, however, finally to the unanimous conclusion which I have announced in my monograph, namely:

(1) *Two or more feeling-elements may coexist in consciousness; they may be like or they may be unlike.*

(2) *Feeling-elements can often be localized*[1].

---

[1] Since the above paper was printed there has appeared from the Psychological Laboratory at the University of Louvain an experimental study entitled "Feeling Experience and its Modalities," by Gerald B. Phelan, M.A., Ph.D., London and Louvain, 1925. As regards the coexistence of feelings this author appears to have come to a similar result as myself, whilst the question of localizability seems not to have been considered.

(*Manuscript received* 6 *April*, 1925.)

THE SOCIAL DISTRIBUTION OF INTELLIGENCE IN THE ISLE OF WIGHT

BY HECTOR MACDONALD.

(From the Department of Education, Armstrong College, Newcastle-upon-Tyne.)

THIS paper is founded on the results of a group Intelligence Test[1] held in the elementary schools of the Isle of Wight on 2 February, 1925. Intelligence quotients were obtained in the case of 2047 children, who would be between the ages of 11 years and 13 years on 1 August, 1925, and these have been grouped in six classes, as follows:

Class	I.Q.	Cases
A +, *A* and *B* +[2]	120 or over	101
B	110 to 119	275
C +	100 to 109	626
C −	90 to 99	519
D	80 to 89	358
D −	Below 80	168
		2047

Where the occupation of a child's parent was unknown that paper was omitted, but a group was formed of children whose parents were dead, though in many cases the occupation of the mother was classified.

The 2047 children were grouped according to their parents' occupation, the average I.Q. of each class being shown. It is to be expected that a parent's status or occupation will be reflected in some measure in the child's intelligence, although this connection is not direct, as the occupation is the parent's and the intelligence is the child's. By 'intelligence' we mean those particular qualities measured by this particular 'intelligence' test which, like all group tests involving reading, undoubtedly measured 'schooling' to a considerable extent, as well as native intelligence.

---

[1] The test was standardized by Prof. Godfrey Thomson and was comparable in difficulty with that held in Northumberland on 24 February, 1922, described in a paper in this *Journal*, 1923, XIV, p. 192, to which this forms an addition.

[2] With reference to this first class, it may be stated that, in the Northumberland test, I.Q.'s of 140 or over constituted class *A* +, and those of 130 to 139 class *A*, whereas in this paper these groups have been combined with I.Q.'s of 120 to 129.

Following the classification of Mr J. F. Duff, who was responsible for the Northumberland list mentioned above, we have grouped the occupations as far as possible to show differences of social standing, of responsibility and of skill.

It has not, however, been possible to do this accurately, owing to the different grades often classified under one heading, *e.g.*, 'engineers,' which is a very vague term. It will also be seen that many groups contain so few names that comparatively little may be inferred from the average I.Q., but later it is hoped, as more and more tests are held, that comprehensive groupings may be formed including much larger numbers drawn from greater areas.

The average I.Q. has been reckoned by taking each class letter at its middle value, thus placing 75 as the lowest possible value and 125 as the highest. This procedure, while accurate enough for the middle classes, will tend to depress the apparent intelligence of a clever group (many of whom would be much above 125), or inflate that of a dull group. It would therefore, in so far as it is inaccurate, tend to a conservative estimate of any class differences which may be present.

The average I.Q. thus calculated for the 2047 children is 98·2. The average I.Q. of each group is given in the following table:

	Occupation of fathers	No. of Cases	Average I.Q.
C[1].	Auctioneers and estate agents	2	116·3
C.	Accountants	5	113·5
I.	Riggers	4	113·1
	*	1	112·5
J.	Upholsterers	7	110·7
J.	Printers and Compositors	9	110·3
A.	*	1	110·0
C.	Wireless signalmen, telegraphists and telephone operators	6	110·0
H.	Plumbers and gasfitters	9	110·0
E.	Watchmakers and jewellers	4	109·4
A.	Musicians	3	109·1
G, L[2].	Farm foremen and farm bailiffs	4	108·8
B.	Managers (miscellaneous)	21	108·7
E.	Hotel and restaurant keepers	11	107·9
J.	Lime burners	2	107·5
E.	Grocers and fruiterers	18	105·4
C.	Timekeepers	4	105·0

[1] The letters in this column show which occupations are combined in the second list and in diagram I.

[2] In the two cases where there is an alternative grouping, class L has been taken and no occupations are included in two groups.

	Occupation of fathers	No. of Cases	Average I. Q.
E.	Hairdressers	5	105·0
J.	*	1	105·0
	Retired, pensioners, independent	16	104·8
C.	Insurance agents	10	104·5
A.	Schoolmasters	3	104·2
E.	Cabinet makers, french polishers, undertakers and wheelwrights	6	104·2
	Ships' officers, pilots	8	103·4
D.	Customs and excise officers	6	103·3
F.	Engineers (unspecified)	31	103·1
G.	Foremen (miscellaneous)	17	103·1
D.	Postmen and post-office sorters	16	103·0
A.	*	1	102·5
	*	1	102·5
E.	Photographers, chemists, dispensers and herbalists	6	102·5
I.	Farriers	2	102·5
K.	Quarrymen	2	102·5
I.	Boatbuilders	9	102·4
F.	Locomotive drivers and firemen	21	102·3
F.	Motor engineers	7	102·1
E.	Butchers and fishmongers	34	102·1
J.	Chefs and cooks	10	102·0
C.	Clerks and draughtsmen	35	101·9
E.	General shopkeepers and assistants	35	101·9
D.	Police and warders	37	101·6
J.	Brickmakers	8	101·6
J.	Tent-, sail- and rope-makers	10	101·5
E.	Tailors	14	101·4
I.	Fitters, turners, drillers, mechanics	27	101·3
L.	Dairy farmers	9	101·1
I.	Shipwrights	9	101·1
J.	Caretakers, watchmen, and commissionaires	12	101·0
J.	Brewery workers	7	100·7
F.	Chauffeurs and motor drivers	39	100·2
E.	Bakers and confectioners	22	100·1
J.	Handymen and odd-job men	6	100·0
J.	Deliverymen	17	100·0
H.	Painters	64	99·8
F.	Electricians	5	99·5
C.	Commercial travellers	8	99·4
C.	Local Government and Urban District Officers	11	99·3
L.	Gardeners	90	99·3
D.	Soldiers, sailors (R.N.), coastguards	52	99·2
H.	Builders and contractors	14	99·1
C.	Bookmakers	2	98·8
	Railway workers (miscellaneous)	28	98·8

	Occupation of fathers	No. of Cases	Average I. Q.
M.	Laundresses and charwomen	25	98·7
M.	Corporation workers	36	98·6
H.	Masons	10	98·3
L.	Groundsmen, park-keepers, etc.	9	98·3
L.	Farmers	41	98·2
H.	Carpenters and joiners	59	98·2
E.	Innkeepers	14	97·9
J.	Cabmen, carriers, carters, carmen and vanmen	50	97·8
H.	Bricklayers and slaters	51	97·6
I.	Boilermakers	13	97·5
I.	Blacksmiths	19	97·4
N.	Seamen, ships' stewards, bargemen, boatmen, fishermen and yachtsmen	60	97·4
M.	Builders, brickyard and quarry labourers	17	97·2
M.	Stokers and boilermen	26	97·1
E.	Marine store dealers	5	97·0
D.	Army and navy pensioners; ex-soldiers and sailors	30	96·7
J.	Porters	9	96·7
	Dead (former occupation unknown)	165	96·6
J.	Nurses, matrons, housekeepers	12	96·5
M.	Semi-skilled workmen	18	96·5
J.	Millers	4	96·3
M.	General labourers	153	96·1
	Invalids (former occupation unknown)	5	96·0
I.	Coppersmiths	7	95·7
E.	Shoemakers and saddlers	17	95·7
K.	Miners	4	95·6
L.	Shepherds	10	95·5
J.	Butlers, waiters, house-stewards	12	95·4
M.	Dock labourers	9	94·7
J.	Slaughterers	9	94·4
I.	Caulkers	3	94·2
I.	Platers and riveters	26	94·1
M.	Window cleaners and bill posters	7	93·9
J.	Sign painters	2	93·8
I.	Moulders	4	93·8
L.	Coachmen, huntsmen and stud grooms	8	93·8
L, M.	Farm labourers and farm carters	83	93·8
M.	Hawkers, sweeps and street musicians	24	93·3
E.	Dairymen and milkmen	22	93·1
J.	Laundry workers	5	93·0
F.	Post-office mechanics	6	92·9
L.	Cowmen	16	92·8
J.	Hospital and asylum attendants	4	92·5
J.	Storemen	5	90·0

	Occupation of fathers	No. of Cases	Average I. Q.
E.	Sundry dealers (horse and cattle, etc.)	11	89·8
L.	Estate workers	7	89·6
	Unemployed	15	88·8
M.	Gravel diggers	9	85·8
F.	Divers	2	85·0
	*	1	85·0
M.	Coal heavers	4	81·9

Occupations of similar standing grouped together:

A.	Professional.	H.	Building trades.
B.	Managers.	I.	Metal workers, shipbuilders.
C.	Higher commercial classes.	J.	Miscellaneous industrial workers.
D.	Army, navy, police, postmen.	K.	Miners and quarrymen.
E.	Shopkeeping class.	L.	Agriculture, all classes.
F.	Engineers.	M.	Labourers and low grade occupations.
G.	Foremen.	N.	Seamen, bargemen, boatmen etc.

* In cases where there is only one member of a class the name of the occupation has been omitted lest identification and embarrassment should ensue. The occupations thus omitted are, in irregular order, professional golfer, actor, veterinary surgeon, piano-tuner, minister, riding master.

	I.Q. <100			I.Q. >100				
	D –	*D*	*C* –	*C* +	*B*	*A* +, *A* and *B* +	Total	Average I.Q.
B	1	1	2	7	6	4	21	108·7
A	0	0	0	6	2	0	8	106·6
C	1	12	20	25	14	11	83	103·3
G	0	4	4	3	1	5	17	103·1
F	6	12	31	38	18	6	111	100·8
E	18	36	44	73	41	12	224	100·7
D	9	24	26	55	23	4	141	99·9
I	8	21	32	45	12	5	123	99·3
H	21	29	50	67	30	10	207	99·1
J	11	35	62	57	22	14	201	99·1
K	0	1	2	3	0	0	6	97·9
N	4	11	20	17	5	3	60	97·4
L	29	57	72	75	36	8	277	96·7
M	35	66	100	82	30	15	328	96·0
	143	309	465	553	240	97	1807	

This result may be expressed diagrammatically thus[1]:

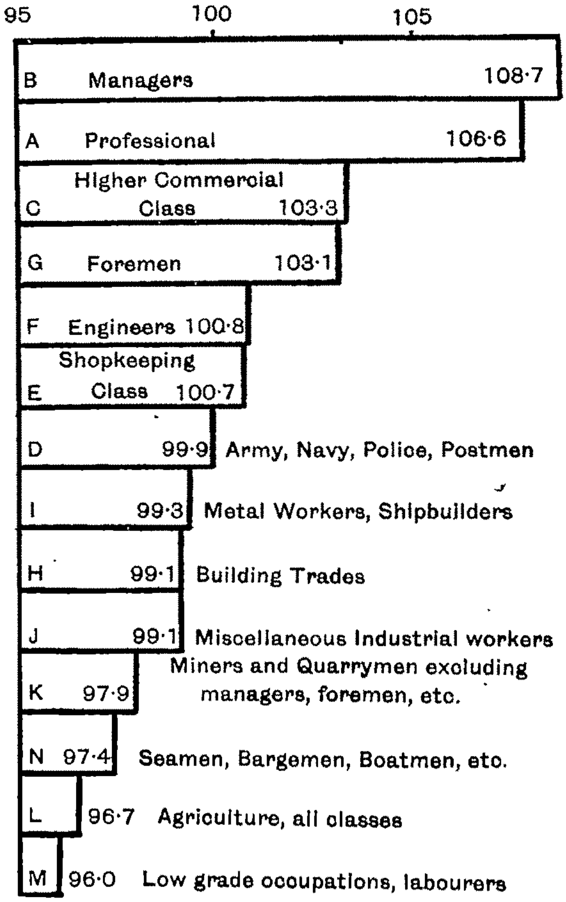


Diagram 1.

The children have also been placed in two divisions: (*a*) those whose parents' occupation is teaching, giving orders, selling and writing; and (*b*) those whose parents' occupation is making, mending, moving, growing or tending any kind of goods. 'Brainwork' and 'Handwork' have been adopted as names for these two divisions, though obviously these cannot be water-tight compartments. 'Brainwork' includes all groups A, B, C, E and G; and also ships' officers, police, customs and excise officers, and one or two other occupations which have not been hitherto classified, *e.g.*, riding master. Four groups, retired, etc., dead, invalids and unemployed have been omitted. The result is as follows (see also Diagram 2).

---

[1] Much overlapping occurs, and though average intelligence quotients show a steady change, no specific predictions can be made in individual cases. The coefficient of mean square contingency is 0·264. In Northumberland the coefficient by Pearson's other, less reliable, method was 0·28.

Brainwork 406 children, average I.Q. = 102·1
Handwork 1412 „ „ I.Q. = 97·3

	D−	*D*	*C*−	*C*+	*B*	*A*+, *A* and *B*+	Total
Brain	22	60	81	135	76	32	406
Hand	121	251	386	421	168	65	1412

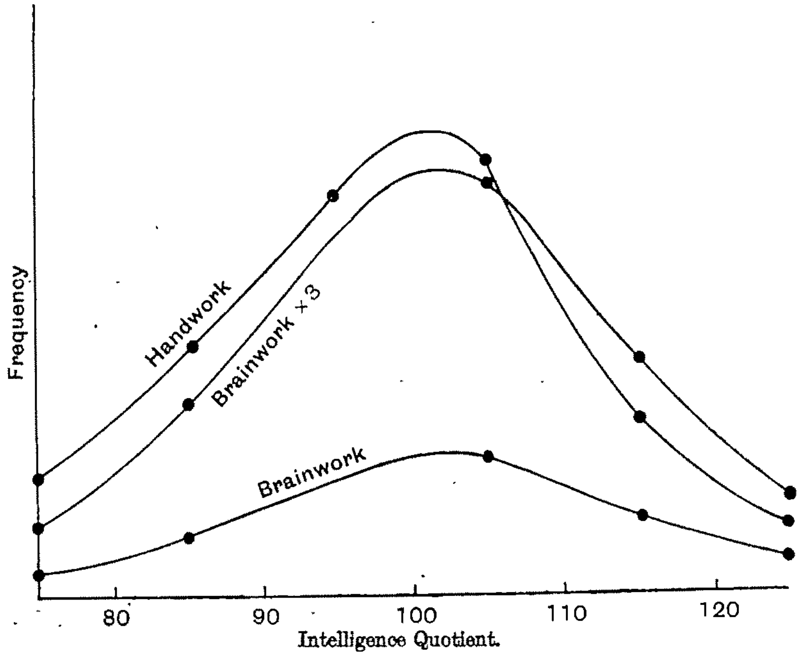


Diagram 2.

(*Manuscript received* 4 *June*, 1925.)

A NOTE ON THE PSYCHO-GALVANIC REFLEX CONSIDERED IN CONJUNCTION WITH ESTIMATES OF CHARACTER QUALITIES

By W. S. BROWN.

(*From the Psychological Laboratory, University of London, King's College.*)

I. *Method of Investigation* (pp. 130, 131).
II. *Treatment of Results* (p. 131).
III. *Results* (pp. 132–135).
IV. *Note on Teachers' Estimates* (p. 135).

The following is a brief note on an investigation carried out in 1923, in a London elementary school.

The object of the research was to obtain evidence regarding the supposed emotional significance of the psycho-galvanic reflex.

I. *Method of Investigation.*

The research consisted of two parts viz.:

(1) *The Estimates of Character.*

These were made by two judges, both class-teachers, working quite independently, on the same lines as in Dr Webb's research on "Character and Intelligence[1]."

The estimates were made under thirty separate headings, most of these being identical with those in the research referred to.

Assessments were made by means of numerical marks, in seven grades from + 3 to − 3. The teachers concerned were given the mark sheets and we discussed together the precise interpretations to be given to the various character qualities assessed. They then had two months in which to observe the boys with a view to the assessments.

The actual estimates were made in February and March 1923, and a normal distribution of marks under each heading was insisted upon.

---

[1] *Brit. J. of Psychol.* Monogr. Suppl. No. I.

(2) *The testing of each subject by means of the psycho-galvanic reflex.*

Six physical stimuli were given. Deflections of the galvanometer and times of deflection were noted. Subjects were seated, with electrodes moistened with normal saline solution on the palm and back of the right hand. The rest of the apparatus was screened off. The subject's resistance was balanced, with the ordinary Wheatstone Bridge arrangement; the stimulus was then given, and the requisite readings taken. The deflections were reduced to show percentage fall in resistance in each case. The stimuli were:

1. A loud unexpected noise (a Klaxon horn).
2. Threat to prick the subject with a pin.
3. Offering sweets.
4. Threat to burn subject with a match.
5. A pleasant scent.
6. Another unexpected noise (dropping a tin box).

II. *Treatment of Results.*

The coefficients of correlation between the estimates for each quality separately were calculated as a measure of the reliability of the assessments. The 'Method of ranks' was used

$$r = 1 - \frac{6\Sigma\,(d^2)}{n\,(n^2 - 1)}$$

and the probable error calculated by the formula

$$\text{p.e.} = \frac{0{\cdot}7063\,(1 - r^2)}{\sqrt{n}}.$$

No assessment was used subsequently whose reliability was less than ·33, which was higher than three times the probable error.

The numerical estimates of the two judges for each subject were then pooled for each quality in the subsequent work.

The total of the percentage drops in resistance for the six tests was then taken for each subject, and the correlations were calculated between this quantity and each of the pooled estimates used. A correction was made for the use of the pooled estimates, using the formula

$$r = r_1 \sqrt{\frac{1 + r_{ab}}{2r_{ab}}}$$

r, being the corrected coefficient,
r_1, the crude coefficient, and
r_{ab}, the coefficient of reliability between the two estimates.

III. *Results.*

The following is the schedule of character qualities estimated. There are slight variations from Webb's list.

A. A preliminary estimate of character under four headings:

1. General excellence of character.
2. General tendency to display emotion.
3. Strength of will.
4. Intelligence.

B. Detailed estimates under the following heads:

1. General tendency to be cheerful.
2. Readiness to become angry.
3. Readiness to recover from anger.
4. Readiness to show fear in presence of physical danger.
5. Readiness to show fear in presence of moral danger.
6. Readiness to display affection.
7. Excitability in any unusual circumstances.
8. Eagerness for admiration.
9. Fondness for companionship.
10. Desire to be liked by associates.
11. Desire to excel.
12. Degree of influence on fellows.
13. Desire to impose his own will on others.
14. Trustworthiness.
15. Bodily activity in school work.
16. Bodily activity in games.
17. Persistence in face of obstacles.
18. Persistence as opposed to mere changeability.
19. Rapidity in making a decision.
20. Tenacity in carrying out a decision.
21. Tendency to show kindness.
22. Conscientiousness, interest in right or wrong of actions.
23. Extent of mental work bestowed on studies.
24. Extent of mental work bestowed on games.
25. Quickness of apprehension.
26. Profoundness of apprehension.
27. Soundness of common sense.
28. Originality of ideas.
29. Sense of humour.
30. Soundness of bodily constitution.

The following table shows the reliability of the estimates and the correlation coefficient between the estimates and the decrease of resistance.

Quality	Reliability	Probable error	Correlation coefficient between Estimates and Decrease of Resistance	Probable error
A. 1	·77	·039	−·04	·096
2	·39	·081	−·08	·095
3	·36	·083	·05	·096
4	·65	·056	·09	·095
B. 1	·56	·067	·05	·096
2	·40	·081	·12	·095
3	·22	·091	—	—
4	·12	·094	—	—
5	−·001	—	—	—
6	·24	·09	—	—
7	·35	·084	·21	·092
8	·42	·080	·16	·094
9	·43	·079	−·05	·096
10	·26	·089	—	—
11	·49	·072	·32	·087
12	·33	·086	·17	·092
13	·45	·076	·12	·095
14	·45	·076	·005	—
15	·47	·074	·20	·092
16	·49	·072	·18	·092
17	·47	·074	·18	·092
18	·42	·080	·21	·092
19	·43	·079	·25	·090
20	·44	·077	·19	·092
21	·23	·090	—	—
22	·13	·094	—	—
23	·43	·079	·085	·095
24	·56	·067	·22	·091
25	·60	·062	·20	·092
26	·47	·074	·12	·095
27	·53	·069	·22	·091
28	·35	·084	·06	·096
29	·51	·071	−·06	·096
30	·39	·081	·23	·091

The reliability coefficients in column two are not high but they are certainly, in most cases, significant, and were considered to justify the continuation of the investigation. They also testify to the independence of the two judgments.

Three months later the judges were handed back their lists and asked whether they would revise any of their markings if asked to make their estimates now. Only a very few minor alterations were made.

The coefficients in column four furnish no evidence for assuming any connection between the character estimates of qualities of an emotional nature and the psycho-galvanic reflex, and so far as the object of the experiment goes:—viz., to establish the connection between the reflex and emotional tendencies, the results are negative.

A closer examination of the figures in column four, however, gives certain interesting indications.

Arranging the figures in column four in order of magnitude we get the following:

	Quality	Correlation between Estimates and Psycho-Galvanic Reflex	
1.	Desire to excel	·32	(greater than three times p.e.)
2.	Rapidity of decision	·25	(greater than twice p.e.)
3.	Soundness of bodily constitution ...	·23	
4.	Mental work on games	·22	
	Common-sense	·22	
6.	Excitability	·21	
	Persistence as opposed to changeability	·21	
8.	Bodily activity in school work ...	·20	
	Quickness of apprehension	·20	
10.	Tenacity of decision	·19	
11.	Persistence in face of obstacles ...	·18	
	Bodily activity in games	·18	
13.	Influence on fellows	·17	
14.	Eagerness for admiration	·16	
16.	Profoundness of apprehension ...	·12	
	Recovery from anger	12	
	Imposing will on others	·12	
17.	Intelligence	·09	
18.	Mental work on studies	·085	
19.	Originality of ideas	·06	
20.	Cheerfulness	·05	
	Will-power	·05	
22.	Trustworthiness...	·005	
23.	General excellence of character ...	−·04	
24.	Fondness for companionship	−·05	
25.	Sense of humour	−·06	
26.	Emotionality	−·08	

It will be noticed that the qualities appearing in the upper part of this table, and correlating most highly with the psycho-galvanic reflex (Nos. 1–11), are mainly those qualities which have an element of 'will,' in the sense of consciously directed activities. The exceptions are soundness of common sense, excitability, and soundness of bodily constitution.

From discussion with the judges since, however, it is clear that the same will-aspect entered largely into their judgment of the first two, whilst the last would be expected to go very closely with qualities of this nature. On the other hand, 'will power' estimated under A 3 in the schedule of qualities is very low in the list. (The explanation appears to be that the judges attempted to estimate this by the various manifestations of will power referred to in subsequent headings, and from my discussions of the subject with them since it seems that conflict arose over the question of will as 'obstinacy' in games and general intercourse with their fellows, and their tendency to persist in school-work. The two qualities did not always go together.)

Further, it will be noticed that those qualities having a predominantly emotional aspect are all very low in the table.

It is not claimed, of course, that the figures are conclusive, but they do seem to indicate a possibility that the psycho-galvanic reflex, if it has a real psychological significance, may be closely connected with these 'will-qualities' or distinctly conative tendencies, rather than with emotions, as has in general previously been asserted.

The disentanglement of the affective and conational aspects of orectic process has always proved of the utmost difficulty, but there may be some indication here that the distinction can properly be made, and it is suggested that further investigation along these lines would yield fruitful results.

In this connection it is noteworthy that some experiments conducted by Miss Wells at King's College, London, and described in this *Journal*[1] point to a somewhat similar conclusion; the two investigations being carried out quite independently and at different places.

IV. *Note on Teachers' Estimates.*

The question of the possibility of character estimation has been much discussed recently. The difficulties appear to be of two kinds, first the difficulty of a satisfactory analysis of character, and secondly the capacity of judges to estimate.

Both the judges concerned in this investigation emphasised the difficulty which they experienced in estimating for those qualities which are not prominently displayed in the ordinary work of an elementary school. "It would be necessary to see the boys in their homes," and "One would need to live with the boys, to make sound estimates under many of these headings" are some of their observations. The result is probably that most of the estimates are unduly weighted by knowledge of scholastic attainments. (The correlation between the pooled estimates of 'General excellence of Character' and 'Intelligence' in this case was ·76—the highest but one in all the work.)

The best conditions for such work would be in a boarding school or residential college of some kind, where the greater part of the lives of the subjects would be open to observation.

In work concerned with elementary day school scholars it would probably be wise to reduce the number of estimates required and to group the qualities in such a way that the teacher in a day-school would have adequate data on which to form a sound judgment.

---

[1] Vol. XIV, pt. 3.

(*Manuscript received* 10 *April*, 1925.)

FURTHER COMMENT ON THE PSYCHOLOGICAL SIGNIFICANCE OF THE GALVANIC REACTION

BY DAVID WECHSLER (*New York*).

IN a recent number of this *Journal* Miss H. M. Wells reported some observations on the psycho-galvanic reaction made in connection with her experiments on choice, from which she was led to conclude that "the occurrence of the reflex...is regularly preceded by changes in consciousness of a conative, rather than an emotional character[1]." This conclusion was based mainly upon the fact that analysis of the introspective data furnished by the subjects' shows "...that they had all been endeavouring to describe consciousness of action...[2]." Aside from the fact that in making so sweeping an assertion, the author has disregarded the experimental results of previous investigators, the conclusion itself appears to me to be a *non-sequitur*, and as similar arguments are not infrequently employed by writers who are discussing the nature of psychic processes said to precede or accompany physiological expressions of the emotions, I believe it is worth calling attention to the error involved.

The argument presented to us may be re-stated as follows: The subjects' introspections on their state of consciousness during the act of making a choice contained no data descriptive of affective reactions; they did contain almost exclusively descriptions of changes of consciousness of a conative character. Hence, the reflex which accompanied these changes is an expression of conative changes in consciousness. Granting for the moment that the introspection revealed no facts that may be considered as descriptive of affective states, it is nevertheless obvious that the author has asked us to make two assumptions: first, that the affective reactions, if they were to occur, must necessarily have been conscious; second, that if conscious they must have been of a nature that could be easily described or as easily described as those she classed as conative. Now neither of these assumptions is warranted by the experimental results either in connection with the psycho-galvanic reflex itself or the other so-called emotion-indicators.

---

[1] This *Journal*, 1924, XIV, pt. 3, 305.

[2] *Ibid.* p. 304.

Among the facts most solidly established by modern researches in psycho-pathology is that emotional reactions (affective tone) may be and in fact are very often unconscious or subconscious. It is sufficient to refer to the work that has been done in this field in connection with the association experiment. Combining the psycho-galvanic technique with the reaction-time and association experiment, both Binswanger[1] and more recently Whately Smith[2] for instance, have shown that the psycho-galvanic reflex is one of the most effective means of calling attention to those non-conscious affective reactions to which the name 'complex' has been given.

To judge the nature of the psychic state on the basis of the verbal description of the conscious state is often misleading. I cite, by way of illustration, a case taken from an association experiment. Exactly the same type of result occurs frequently where other than association words are used as stimuli. In the course of an experiment a young lady responded to the stimulus word 'carry' by the reaction word 'book.' In introspecting her mental content at the moment she said that she could record nothing in particular and no emotional reaction whatsoever. I asked her to recall the word again and to use it as the beginning of a chain of free associations. Her first response was that the word 'book' recalled a certain novel. This, in turn, reminded her of the friend who had loaned her the book. The friend was married and had recently given birth to a child. She now pictured her friend pushing a baby in a go-cart. I will stop here for it is clear to me that the word 'carry' was unconsciously associated with 'baby' and that the affective reaction, as indicated by the galvanic reflex, was due to the association of the words 'baby' and 'carry.' The subject's description of the mental state experienced at the moment of her reaction gave no inkling of this. The introspective data without analysis, taken at its face value, was in no way descriptive of an affective process and should we rely on this alone, we might justly maintain that no affective reaction had occurred. It is clear, however, that in the case cited there must have been such a reaction.

Results of the studies on the psycho-galvanic reflex in connection with the association experiment have furnished strong evidence that the psycho-galvanic reflex is primarily an index of subconscious affective reactions (Jung, Binswanger *et alt.*). However that may be, it has been shown beyond doubt that the psycho-galvanic reflex is not dependent upon conscious cerebration. This follows, not only from the results obtained in connection with the association experiment, but from the fact that the reflex may be obtained under hypnosis (Prince, Moravcsik), that it can be evoked by stimulation of analgesic or anaesthetic regions of the skin (Veraguth), and from other evidence. Braunschweiler and Veraguth obtained the reflex with subliminal stimuli[1], and I have myself obtained galvanic responses from subjects during sleep[2].

---

[1] L. Binswanger, "Ueber das Verhalten des psychogalv. Phänomens beim Assoziationsexperiment," *J. f. Psychol. u. Neurol.* 1908, x, 149; and xi, 65, 133.

[2] W. Whately Smith, *Measurement of Emotion*, New York and London, 1922, ch. III.

Even when present in consciousness, however, affective states may escape introspection, especially when they are feeble, as they must have been in the case of those evoked by the type of stimuli employed by Miss Wells. It is only when affective reactions reach a certain intensity that they are attended to and observed and then, as is well known, owing to the difficulty in describing them they are, as a rule, only vaguely reported in introspective protocols. I think it is reasonable to assume that if affective states were first mildly presented in the subjects' consciousness it is quite possible that owing to their feeble intensity they were disregarded. It is interesting to note, however, in this connection that the author reports that when the choices involved were difficult the magnitude of the galvanic reactions recorded were proportionately greater—that is, they were more significant when, as there is every reason to believe, the emotional reactions accompanying them were more marked.

Finally, I have inspected the actual protocols furnished, and it appears to me that they are not as free from introspections which may be considered descriptive of affective processes and states, as the author leads us to suppose. The following remarks for example are recorded: "swinging of attention with *feeling of* activity"; "feeling of mental *strain*"; "there are also muscular feelings throughout from eyes and forehead"; "notice also tightening of upper eyelids and some *physiological sensations*"; "I could not localize the extent since a *bodily state breathing*, etc. etc.[3]"

Anybody who has attempted to analyze protocols of affective states will recall how frequently exactly similar descriptions of muscular strain, mental tension, and the like occur. Of course, the majority of the introspections given by the author are those of action or description of action. This was simply because the type of stimulus employed

---

[1] That is stimuli that were not consciously perceived by the subject.

[2] These experiments are as yet unpublished.

[3] *Loc. cit.* pp. 302 and 303 (italics mine).

(choices) naturally favour descriptions in conative terminology. If the author had studied the psycho-galvanic reflex in connection with the mental work involved in the solution of problems in arithmetical reasoning she would have found the introspections largely cognitive; if sensory stimuli had been employed much of the introspection would have been descriptive of various kinds of sensory imagery.

In conclusion, I would say that Miss Wells' data strike me as having furnished, to the very contrary of her own conclusions, some further evidence for the affective nature of the galvanic reaction. Her experiments, in fact, point out a way in which the galvanic response may be employed to measure the affective tone that accompanies some of the higher perceptual processes. Miss Wells' error consists in assuming that affective reactions necessarily give rise to some conscious feeling states. The occurrence of the reflex is not necessarily preceded by anything that is conscious and a description of the state of consciousness at the moment of its occurrence does not necessarily give an indication of the nature of the psychic processes that may have given rise to it. The same is true of any other of the so-called physiological expressions of the emotions.

(*Manuscript received* 20 *April*, 1924.)

PUBLICATIONS RECENTLY RECEIVED

The Psychology of Time. By MARY STURT. London: The International Library of Psychology, Philosophy and Scientific Method: Kegan Paul. 1925. pp. vi + 152. 7*s.* 6*d.* net.

This slender volume provides a brightly-written and readable introduction to a very perplexing set of problems. Opening with a rather sketchy discourse on the 'metaphysical view of time,' Miss Sturt proceeds in the following chapter to trace the origin of our time experience. This, as the author herself recognizes, is inevitably a speculative undertaking; but her own account, one thinks, would have been improved had she distinguished more clearly between ontogenetic and phylogenetic considerations.

The third chapter, dealing with the social organization of time, contains some interesting details regarding the time-reckonings and nomenclature of primitive peoples. In this connection Miss Sturt accepts Nilsson's view that the 'day' of 24 hours is a [relatively] late conception for which extremely few languages possess a word. But if the latter part of this assertion is intended to be evidence for the former, the inference is inadmissible; arguments from the absence of a word to the absence of the corresponding conception are obviously quite worthless. On the other hand, the philological case itself seems to be of doubtful strength. While it is true that most modern Indo-European languages lack the required specific term (the Scandinavian group forming the main exception), many other tongues do possess a word for the period in question which is not a compound of the words for 'night' and for 'day.' Arabic and the Semitic languages generally are notable examples: and it is certain that the Arabic term for the '24-hour' period is more primitive than the term which means 'day as distinguished from night.' One may also cite, among Central African tongues, the Hausa word *kwana*; or, again, in the Bantu group, the Ila *bushiku* and the Baganda *lunaku*, standing for the interval which is reckoned from one sunset to another. The latter, indeed, would appear to constitute the natural limits of a well-marked period, especially in equatorial regions where there is practically no twilight.

The next two chapters (IV and V) incorporate experimental results already published in this *Journal* (vol. XII, part 4; vol. XIII, part 4). These investigations were designed to ascertain the knowledge of temporal distinctions possessed by young children and the ability of both children and adults to estimate duration. In the latter case the author studied the influence on time-estimates of conditions of comfort and discomfort and of free and fixed attention. The situations chosen were sometimes very enterprising (as *e.g.* 'a stuffy railway carriage with a screaming baby in it'); but the data presented strike one as in need of considerable supplementation, and it is to be hoped that Miss Sturt will continue these and similar experiments on a more varied and extended scale. Her general conclusion on the subject of duration (chaps. v and vi) is that in estimating time we depend not on hedonic factors but on the 'amount of mental content' (the number and nature of the mental events) which occur within the given period.

In her discussion of the order of past, present, and future (ch. vii), Miss Sturt rejects the views of Fouillée and Ward that this order depends [in part?] on the relative vividness of different parts of the memory-train. She rejects also Ward's theory of the temporal function of 'movements of attention'; but her criticism here seems open to question. Granting that the factors adduced by Ward cannot be the sole factors operative in determining the order of events in time, it certainly does not follow (as the author suggests) that they play little or no part in the matter. And the facts themselves would appear to support the contrary conclusion. Even at the perceptual level we have what Stout has called a 'not yet' consciousness and a 'no more' consciousness. The experiences thus indicated are essentially of the nature of attitudes of attention, and they probably lie at the root of our more developed conceptual constructions.

In the concluding chapter the author returns to the general treatment of the nature of time which was begun in the first. Her discussion contains a good many disputable dicta on the meaning and status of the temporal experience and, on the whole, is not very satisfactory. The complexities of the theme before us really demand a much fuller and more thoroughgoing analysis than is attempted in this book. But, admittedly, the subject of time has hitherto been too largely neglected by psychologists; and in raising the issues afresh Miss Sturt has performed an opportune service.

A few misprints have been noted: pp. 3, l. 19; p. 119, l. 10; p. 128, l. 6; and footnote references on pp. 105, 110, 112, 126.

W. H. O'N. M.

An Introduction to the Psychology of Religion. By ROBERT H. THOULESS. Cambridge University Press. 1922. Second Edition 1924. pp. vii + 286. 7*s.* 6*d.* net.

This book was compiled from lectures delivered to ordination candidates, and, considering that the author's sympathies seem to be on the side of a religious explanation of religious phenomena, it is admirably free from prejudice. It could not possibly offend anyone's susceptibilities. We are given a helping hand over every stile which looks a little difficult and uncomfortably tall, but we are made to face it. The relation of sex to religion is duly dealt with, but we are rather too carefully told that sex is not the whole of the matter. The little pill of comfort, and the practical maxims at the end of many of the chapters may be irritating to many readers, but it is often necessary to bring in irrelevant remarks in order to quiet irrelevant objections, when dealing with religion from a purely psychological point of view. For this reason the book may be recommended, particularly to such persons as are likely to be made uncomfortable by the cruder treatment of some writers on the subject, and who nevertheless are interested in a semi-objective way in the extraordinary phenomena it provides.

Into his 286 pages Dr Thouless has packed a very great deal of matter. He differentiates five important factors, any of which may cause religious belief-feeling: tradition, nature, ethics, emotion, and reason. These are discussed separately. There follow five chapters on Psychology in general, and the sex and herd instincts in particular. These chapters are simple and adequate for the purpose. We now come to the phenomena of religious experience themselves—to prayer, conversion, and mysticism, with a fascinating account of a modern mystic (by far the most important chapter for the professional psychologist). The last chapter is an orgy of comfort, but contains some curious arguments which have been adduced in favour of the religious explanation. These arguments seem to refer to the Christian religion —a discrete silence is preserved about other possible theistic formulations. The oddest of the arguments is that God cannot be a phantasy because direction of libido towards him is found to solve erotic conflicts while phantasies are supposed to be unsuccessful for the purpose.

Dr Thouless has said nothing new, but his matter is arranged better than is the case with any other work on the same subject, and contains copious references to more detailed writings on the various topics involved. An important advantage of the book is its brevity; other writers give more material from the experiences of mystics and converts but, however interesting reading they may afford, one is apt to be unable to see the pattern for the data. W. J. H. S.

Lehrbuch der Psychologie. Von FRIEDRICH JODL. Stuttgart u. Berlin: J. G. Cotta'sche Buchhandlung Nachfolger. 1924. 5u. 6 Auflage. 2 Bde. S. xix + 394; xii + 498. M. 20.

This new issue of Jodl's well-known work fulfils the intention which the author had before him at the time of his death in 1914. Since its first appearance in 1896, the book has been twice revised (1902, 1908) by Jodl himself; but the rapid growth of psychological enquiry in recent years led him to the conclusion that, without the cooperation of younger specialists, his survey could not be kept thoroughly up to date. In accordance, therefore, with his known wish the present revision has been carried out by a small group of pupils and friends, under the general editorship of Prof. Carl Siegel of the University of Czernowitz. In scope the work follows the main lines of most orthodox text-books and compares very favourably with the best books of its kind. It consists of two parts, a general and a special. The former comprises three chapters dealing respectively with the subject-matter and methods of psychology, the problem of body and soul, and the nature, functions, and development of consciousness. The remaining nine chapters, making up the special part, discuss at length the fundamental topics of sensation in general and the special sensations, the affective and volitional phenomena of the primary order, the secondary phenomena of reproduction (memory, association, etc.), and the chief reproductive constructions (time, space, and the distinction between self and not-self), language and the development of thought, and, lastly, the feelings and volitional phenomena of the secondary and tertiary orders. In the present editions the work has been largely re-written; though its total length remains approximately the same as in preceding issues. The most important change from the first edition is in chapter x (on Language and Thought) in which the existing three sections have been rearranged and a fourth section added, on 'the natural movement of thought.' The admirable bibliography which was a feature of the original book has been greatly enlarged (60 pages) and a much-needed index is now supplied.

W. H. O'N. M.

Principles of Psychology. By Prof. J. R. KANTOR. New York: A. Knopf. 1924. pp. xix + 473.

The arrangement of this book—which is apparently Volume I of a more extended work—is interesting. After an introduction dealing with the domain, method and development of psychology, and a chapter on reactions considered as primary data, Prof. Kantor at once embarks on a study of personality. This leads him to consider the formation of systems of reactions, and this, in the end, to a study of social conduct. Only after he has dealt with all these matters does the author discuss those types of common cognitive reactions—sensing, perceiving, processes of association and the like—which most text-books of psychology thrust upon the student at a very early stage. Later still feeling and volitional reactions are dealt with, and the last chapter of all concerns the integrative nature of habit reactions.

Clearly the scheme of treatment has been very carefully thought out. It is one of great interest in itself, and seems highly likely to promote both thought and understanding in the student. Unfortunately the actual presentation is marred throughout by a facility of language and idea which is constantly running into verbosity. The book is like an excellently well-formed person dressed in thick, woolly, and drab clothing. It is a pity that the ideas presented are not more attractively and concisely set forth, for they are very good.

The Dawn of Juvenile Literature in England. By Dr G. ANDREAE. Amsterdam: H. J. Paris. 1925. pp. 122. Fl. 2.50.

In this happily phrased and most interesting monograph Dr Andreae presents a brief and clear study of the growth and development of the child's book in England. Beginning with an account of Locke's views on education, and with a survey of the place of childhood in the poetry of the first half of the eighteenth century, the author goes on to discuss and describe the early books for children and the ideas which inspired their publication. There is an interesting account of the activities of Mr John Newberg who published many books for children about the middle of the eighteenth century, and a brief story of the growth later of 'the moral tale *par excellence*.' The remarks about the praise and abuse of the fairy tale are sensible and to the point. It is perhaps a pity that Dr Andreae has not allowed himself more space for his work, for what he has done he has done well, only there is not much of it. But that is an uncommon fault.

A Study of the Upper Limits of the Development of Intelligence. By Dr FLORENCE M. TEAGARDEN. New York. 1925. Teachers' College, Columbia University, Contributions to Education. No. 156. pp. vi + 112.

The monograph is well-conceived, well-written, and excellently produced. It deals with questions of very great general interest to all who are concerned with educational theory and practice, and deals with them well. Dr Teagarden carried out a survey of current literature dealing with the upper limits of intelligence and in addition obtained a large number of experimental results from 408 individuals with ages ranging from 12½ years through 20 years. She finds considerable evidence for the continuation of the development of intelligence beyond the age limits which many writers have assigned to it, together with a somewhat increasing variability with age. On the whole her survey and her test results justify the view that the

I.Q. remains relatively constant with increase of age. As to the shape of the curve of mental growth, for the cases here studied "the evidence is unmistakeably for a line of fairly even growth with negative acceleration." The author is admirably cautious about the nature of intelligence. This work deserves a wide circulation.

L'Odorat. Par H. ZWAARDEMAKER. Paris. 1925. Gastor Doin. Bibliothèque de Psychologie Expérimentale. pp. 305. 15 fr.

It goes without saying that this book by Prof. Zwaardemaker is one which ought to be read carefully by all psychologists and physiologists who are interested in the study of the special senses. The book is comprehensive, dealing with the physics, the chemistry, the physiology and the psychology of smell. An excellent account is given of the chief researches which have been carried out, and the author is lucid and just in exposition and criticism. Something more might have been done to elucidate the close connection between smell and some of the higher mental processes. But that is undoubtedly at present a field of speculation rather than of secure knowledge. The definitely experimental work on smell is here surveyed by one who is indisputably the master of his subject.

The Fundamentals of Statistics. By Dr L. L. THURSTONE. New York: The Macmillan Company. 1925. pp. xvi + 237. 8*s*. 6*d*. net.

This book is the "result of seven years' teaching the fundamental principles of statistics and mental measurement" to students who have had little or no previous mathematical training. The outcome is a clear, straightforward exposition of the elements of statistics; *e.g.* Frequency Tables and Diagrams; Representative Measures, and Measures of Variability; Probability Curves; Probable Error; Correlation Coefficients, etc. The book can be recommended with confidence. It will be of special value to those who desire to make a first study of the subject by themselves.

Diagnostic Testing and Remedial Teaching. By Prof. E. MARION PAULER. London: D. C. Heath and Co. 1924. pp. xvii + 371. 7*s*. 6*d*. net.

The question is: How may the class work of school children be assessed in relation to the main aims of teaching certain kinds of subject-matter? The book is essentially intended for practical use. It deals first with methods of grading school work, and with certain points of statistical significance. Then it takes, in turn, spelling, writing, composition, reading, arithmetic, geography, history and some high school subjects suggesting methods of assessment in each case. Every chapter is prefaced by a number of questions, such as—on writing—"Which is the most legible handwriting, vertical, semi-vertical, or slant?" "Compare three samples of perfect penmanship in which is used the same slant. Do these samples show the writer's individuality?" The chapters attempt to answer the questions, and lead up to further 'exercises' on the answers. Selected bibliographies are appended to each chapter. It is difficult to imagine anybody reading the book straight through, but there is much to be said for it as a practical reference book for teachers.

Psychological Tests of Mental Capacities. By Prof. A. S. WOODBURNE. Madras: Government Press. 1924. pp. iv + 232. R. 2.8.

Here is presented a general account of the mental test movement "for the information of the teachers of South India of this most important phase of educational psychology." The book is good and clear, and deals with the usual intelligence tests, with performance tests, with vocational and character tests, and with the applications of tests to groups. It presents a history of the movement, discusses methods of testing, deals briefly with the statistical study of results, and considers a number of practical problems which now confront the Indian Educator. As a piece of pioneer work it deserves high commendation.

Mind and Medicine. By Prof. T. W. Salmon. Columbia University Press. 1924. pp. 33. 5*s.* net.

This is a reprint of an address delivered by Prof. Salmon at the opening session of the College of Physicians and Surgeons at Columbia University in 1923. While it does not contain anything very new, it is a clear, balanced and forceful statement of the need for psychological study in medical science. Prof. Salmon was speaking of conditions in America, but all that he says could be repeated with equal applicability to the medical conditions in this country.

Report on the Present Position of Vocational Guidance and Vocational Selection. By M. Boole Stott with an Introductory Note by Dr Beatrice Edgell. London. 1925. The Women's Employment Publishing Company, Ltd. pp. 95.

This survey will be extremely useful to all persons who are interested in the problems of vocational guidance and selection whether from a practical or from a theoretical point of view. The whole is divided into five parts. First comes a general discussion of the questions involved and of the need for original investigation. Part II deals with work done on intelligence and character in relation to the vocational aptitudes of college students. Part III describes experiments carried out upon vocational selection in regard to specific occupations. Part IV contains an account of the institutes and organizations which are concerned in the work of vocational guidance and selection in various countries, and Part V summarizes the main conclusions arrived at up to the date of publication. Most space is, perhaps naturally, accorded to the work done in English-speaking countries, and a supplement dealing particularly with the work attempted in Germany since the war would be welcome. But the monograph is clear, definite, and timely, and should be widely consulted. The Central Employment Bureau for Women is to be congratulated on their enterprise in taking up the investigation, and the author upon the workmanlike manner in which she carried it out.

Manuel de Psychiatrie, sixième edition revue et augmentée avec 4 planches hors texte. By J. Rogues de Fursac. Paris: Libraire Félix Alcan. 1923. pp. xvi + 906.

The sixth edition of this well-known text-book contains a very considerable amount of new material. Part of this concerns a more extended study of certain special psychiatrical problems, particularly those connected with the nature, detection and effects of various syphilitic disorders. Practical psychiatry, so far as it concerns institutional work, receives extended study, and much more is said than before on the legal aspects of work in psychopathology. At the same time there is little evidence that the author has profited from the work of recent foreign psychopathologists. The manual is certainly comprehensive in the sense that it covers a great deal of ground, but hardly so in the sense that it affords a reasoned survey of a number of divergent views.

Sex and Civilization. By PAUL BOUSFIELD. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1925. pp. vii + 294. 10*s*. 6*d*. net.

In this book Dr Bousfield tries to show the sources of the many current and widespread notions in regard to the inferiority of women. His work is in part a venture into comparative anthropology and in part a treatise on psycho-analytical lines, with the latter interest preponderating. The book is not descriptive only. The author believes that he can point the way to a truer equality of sexes than has yet been attained, and in consequence to a more stable organization of society. His problems are undoubtedly of outstanding importance for the social psychologist. The treatment is frank, and often illuminating. But the book would have gained if less had been yielded to popularity, and more genuinely critical research had been attempted.

The Psychology of a Musical Prodigy. By G. RÉVÉSZ, Director of the Psychological Laboratory, Amsterdam. London: The International Library of Psychology, Philosophy and Scientific Method: Kegan Paul, Trench, Trubner and Co., Ltd. 1925. pp. ix + 180. 10*s*. 6*d*. net.

This book contains an account of the capabilities of a young musician in certain musical and intellectual fields at various ages up to his eleventh year. As a psychological study it is disappointing.

Problems of Belief. By F. C. S. SCHILLER. London: Hodder and Stoughton. 1924. pp. vii + 194. 3*s*. 6*d*. net.

Dr Schiller's little book is both vigorous and attractive, but it is not very psychological in its point of view. The definition: Belief is "a spiritual attitude of welcome which we assume towards what we take to be a truth," indicates the kind of impression which the book is constantly making. There is a readiness to adopt a view which is half psychological and half philosophical and a tendency in regard to the psychological part to leave very complex mental states and processes wholly unanalysed. Still, many exciting and provocative things are said.

Freedom of the Mind in History. By H. O. TAYLOR. London: Macmillan and Co. 1923. pp. xii + 297. 7*s*. 6*d*. net.

In the lectures which were the basis of this book, Dr Taylor proposed to study "the free action of the human mind through history, and to distinguish this agency from the grosser or more palpably determined factors shaping the fortunes of our race." He has carried out his task with severity, dignity and a belief in the great value of the destinies of the human mind.

Reason and Morals: an Enquiry into the First Princples of Ethics. By Dr ISRAEL LEVINE. Glasgow: Maclehose, Jackson and Co. 1924. x + 177. 6*s*. net.

Although this lucid little book is addressed to the moralist rather than to the psychologist, the latter will find in it much to interest him. It is maintained that the moral life really grows out of the social life, and draws its sanctions and its content from thence. The view that it is by virtue of reason that a man is able both to appreciate and to observe moral obligations is very well argued, but it might have been developed more fully.

Intelligence in Expression: with an Essay—Originality of Thought and its Physiological conditions. By LEONE VIVANTE. Translated by Prof. BRODRICK BULLOCK with foreword by Prof. H. WILDON CARR. London: The C. W. Daniel Co. 1924. pp. xi + 205.

This book is wholly philosophical in its outlook. The chapter on the physiological conditions of synthesis, and the essay on the physiological conditions of original thought seem to be physiological only in the sense that they speak generally of 'cerebral traces' and 'cerebral mechanism.'

The Philosophy of Character. By Dr EDGAR PIERCE. Cambridge: Harvard University Press. 1924. pp. xi + 434. 20*s*. net. (London: Mr Humphrey Milford.)

Psychology does not occupy much of the space of this book. The author attempts a very wide survey of the various positive sciences and of the whole of reality in an attempt to set up a monadistic theory in which purposive action is the clue to the problems of the universe. In his general psychological position he naturally objects to Behaviourism, and looks with favour upon McDougall.

The Metaphysical Foundations of Modern Science. By Dr E. A. BURTT. London: The International Library of Psychology, Philosophy and Scientific Method. Kegan Paul, Trench, Trubner and Co., Ltd. 1925. pp. ix + 349. 14*s*. net.

Dr Burtt deals in detail with the metaphysics which he believes to underlie the work of Copernicus and Kepler; of Galileo; of Descartes; of the positions of the seventeenth century English Philosophers; of Gilbert and Boyle; and of Newton. His conclusion shows the need for a very critical examination of such underlying notions. The writing is vigorous and interesting.

NOTES ON RECENT PERIODICALS

Journal of Experimental Psychology.

Vol. vi, No. 1.

Habituation to Rotation. (Raymond Dodge.)

Photographic records of nystagmus and head movements during rotation were obtained and are reproduced. A study of the effects of protracted rotation with closed eyes showed the development of 'refractoriness' as adaptation to stimulus. With very slow acceleration, every vestibular datum of rotation after the first—delayed—one was false, and illusions of cessation to rotation followed by that of reversal of direction with positive and negative after-images suggested evidence for a central compensatory mechanism. The operation of inhibition as a force always set in motion by a stimulus in addition to the normal conscious accompaniment (reciprocal innervation of antagonistics in physiology) was provisionally confirmed by rotation experiments with open eyes: where vestibular equilibrium indicated rest, obvious motion was referred to the environment. The author hypothesizes central compensation producing central refractoriness. Records of eye-movements during oscillation as during repeated rotation show a true habituation process, and the similarity of the curve of the development of refractoriness to the typical learning curve leads to the supposition of a common fundamental neural factor. "Education is in fact largely made up of the elimination of undesirable responses."

A Study of Esthetic Judgments. (K. Gordon.)

Judgments of 207 subjects on two series of 50 coloured plates of oriental rugs showed wide diversity of individual judgment, fair stability of individual preference, positive though not high correlation of individuals with groups (in cases where this was high for one series it was so also for the second), large measure of agreement between group and group, increasing with size of group.

A Comparison of the Scale-of-Values Method with the Order-of-Merit Method. (E. S. Conklin and J. W. Sutherland.)

This study indicates the greater suitability of the former for "obtaining a record of judgments or reactions influenced by the immediate emotional response to the situation"; otherwise they are approximately on an equality.

Performance Tests for Three-, Four-, and Five-Year-Old Children. (H. T. Woolley and E. Cleveland.)

These tests were made on 150 children with Montessori apparatus—building of pink tower, and fitting of three sets of cylinders—and showed that they satisfactorily measure abilities of 4 and 5-year-olds, but select only superior 3-year-olds, for whom last five blocks of pink tower made a good test. A 3-minute limit was found ample.

An Experimental Study of the Influences of Organisation of Material for Memorising upon its Retention. (D. A. Laird, Herman Remmers, and L. J. Peterson.)

The experimenters found that their studies bore out the conclusion that learning is specific, not general; variations in recall are determined in part by the particular material studied—nonsense syllables, geography, sums, etc. Other conclusions were as to the importance of organisation and meaningfulness, especially in delayed and organised recall.

A Note on the Conditioned Eyelid Reaction. (Hulsey Cason.)

For greater clearness than in a former paper on the subject, data are given for relative comparison between conditioned and voluntary reaction time, indicating characteristics of the reflex in the former.

Vol. VI, No. 2.

Experimental Studies of Two Important Factors underlying Masculine Sexual Behavior: The Nervous System and the Internal Secretion of the Testis. (C. P. Stone.)

An up-to-date summary of literature bearing on the above.

Thresholds of Rotation. (Raymond Dodge.)

Without prejudice to the relative importance of the reception fields of vision, kinaesthetics, audition, etc., with regard to sensations of rotation, only that of the vestibular mechanism was investigated in this series of experiments, with the exception of audition: the illusion of rotation and corresponding nystagmus was in certain cases produced by auditory stimuli only. The main series showed that with regard to direct orientation in the air, vestibular data from rotations of less than 2° per second are quite unreliable, and are often false for a speed of 4°. The threshold for rotation of rapid onset seemed to be between 1° and 2° per second. The experiments were particularly useful in indicating the most important vestibular problems in flying, and conditions for investigation of an absolute vestibular threshold.

The Relation between Field Brightness and the Speed of Retinal Impressions. (P. W. Cobb.)

The third paper of a series in which retinal sensitivity is investigated by measurement of the shortest time of exposure of a stimulus necessary for discrimination between stimulus and no stimulus. Here seven subjects were used and the following result obtained: "In general, if t represent the threshold time of stimulation, and B the brightness of the experimental field, the relation followed an equation of the form: $1/t = k \log B/B_0$, where k and B_0 are constants depending upon the individual and upon other experimental conditions, notably upon the solid angle subtended by the stimulus-spot at the eye of the subject"—a statement in agreement in form with the laws governing critical flicker-frequency for intermittent stimulation.

The Variability of an Individual in Repetitions of the Same Task. (E. L. Thorndike.)

The general result of tests showed that the variations of an individual are as a rule distributed symmetrically about his central tendency, big drops during ordinary conditions of health being so rare as to be almost negligible.

No. 3.

Adequacy of Reflex Compensatory Eye-movements including the Effects of Neural Rivalry and Competition. (Raymond Dodge.)

This third paper of a series illustrated with photographic records of eye-movements seeks to answer the question "Does the vestibule function as a quantitative spatial reflector for eye-reflexes?" Visual pursuit nystagmus and pure vestibular nystagmus are compared by means of an environment moveable at will in the same or the opposite direction as that in which the subject rotates. After-nystagmus was proved to be a nystagmus of deceleration. The recorded facts show that while vestibular reflex deviation of the eyes precedes and initiates all compensatory eye-movements incidental to rotation it is seldom adequate; for adequate visual pursuit compensatory control of the eyes passes from the basal ganglia to the cortical areas. The records give a very clear picture of rivalry and conflict in the control of the final common neural path where the vestibular reflex is opposed by an unnatural movement of the visual field.

Constancy of Affective Judgment to Odours. (P. T. Young.)

Experiments showed variation of quality and intensity of feeling even with two inspirations of one period of stimulation, the greatest variation during five weeks in connection with indifferent odours, and least with those arousing pronounced affects, also the effect of associative tendencies on individual differences.

Some Factors determining the Order of Elimination of Culs-de-sac in the Maze. (C. J. Warden.)

Type of *culs-de-sac* and not their number or position relative to food-box seems the most important factor in a rat's difficulties in learning a maze. The relative number of entrances made correlate perfectly in rank with the temporal order of elimination; this is determined in large part by differences in the character of the motor co-ordinates required in the avoidance reaction pattern of the different types of *culs-de-sac,* and preliminary suggestions are made towards the interpretation of these differences.

Bisection of Tonal Intervals smaller than an Octave. (C. C. Pratt.)

Experimental results in direct contradiction to those of Lorenz were obtained by a modified use of the method of equal sense distances, the point of bisection of intervals smaller than an octave falling nearer the geometrical than the arithmetical mean. No interpretation is attempted of the logarithmic relation which seems to hold between stimulus and mental process.

Accommodation and Convergence under Low Illumination. (H. E. Israel.)

Experiments showed that near the lower threshold for vision, accommodation alone breaks down, while convergence shows little change in gradual increase of error; also that fatigue has greater effect on accommodation than on convergence. Both showed distinct improvement with practice.

Device for Low-Potential Current in the Psychologial Laboratory. (D. A. Macfarlane and J. S. Roon.)

Changes in Motor Control and Individual Variations under the Influence of "Razzing." (D. A. Laird.)

The generally recognized effect of 'razzing' (cheers, cat-calls, booing etc.) on players in the field was subjected to a series of motor-tests (tapping) with eight 'fraternity pledges' under conditions of friendly competition. Steadiness was always diminished by 'razzing' but that involving body as well as arm muscles was altered most; co-ordination suffered less than steadiness, rate and fatigue of tapping least of all, although all three showed individual differences. The fatigue index of tapping under normal conditions seemed the best indicator of the performance of the individual under 'razzing.'

Vol. VI, No. 4.

A Behavioristic Experiment on Inductive Inference. (Zing Yang Kuo.)

The experiment referred to, of particular importance for the development of technique, endeavoured to "bring to light the different modes of language response involved in the processes of inductive inference, *i.e.* to study by means of what was ostensibly a memory experiment, the spontaneous discovery of the common structure of compound Chinese characters and the relationship which this common structure represented." The objective meaning of the term 'inductive inference' is clear from the above. Overt language reactions of the subject were accepted at their face value. It was found that discovery of radicals was not necessarily accompanied by discovery of their meanings, and the process of inductive inference was classified into five aspects, each further subdivided into types: (1) reaction to relationship, (2) number of evidences, (3) the nature of the hypothesis, (4) the detection of negative instances, (5) the attitude towards the negative instances. A good 'memorizer' of characters was not necessarily a good 'discoverer' of relationships.

The Influence of Likes and Dislikes on Memory as related to Personality. (D. A. Laird.)

As the result of a test originally devised as a class demonstration the author was obliged to abandon his belief in "the oblivescing power of the unpleasant." Temperamental difference in the recall of the pleasant or unpleasant was found to be very marked indeed.

The Resistance of Infants and Children during Mental Tests. (D. M. Levy and S. H. Tulchin.)

This paper emanating from the Chicago Institute for Juvenile Research is founded on experiments carried out in the different conditions of "Better Baby Conferences" in County Fairs and a nursery in a private home; results were fairly uniform, and not materially affected by changed technique, new tests or different examiners. They show that "the manifestation of resistance by infants and children during mental tests is evidence of some innate behavior pattern. It is typically pronounced at 18 to 23 months in females, and at 30 to 35 months in males. It yields gradually with age in both sexes."

Psychological Review.

Vol. XXX, No. 1.

Repression, Release and Normality. (J. S. Mursell.)

An attempt to give an account of the psycho-analytic concepts of the above in terms of general psychology. The author explains repression along the lines of a combination of Kempf's and Watson's views—autonomic disturbances and habit sets, brought about by an environment at once stimulating and regulating. He considers that in psycho-analytic treatment repressions are not released but 'conquered,' and that sublimation is a misleading idea, the normal personality having no important repressions or 'habit-distortions.'

An Extension of Pillsbury's Theory of Attention and Interest. (H. F. Adams.)

After following further Pillsbury's classification of attention conditions and giving a 'quantitative' account of attention (it equals strength of stimulus *minus* amount of nerve energy used in crossing synapses *plus* amount of activity going on in the receptive region of the cortex), the author considers the connection between interest and attention regarded as a relating activity—the bringing together of the incoming and what is already there. He finds that interest results only when attention connects the unknown with the already known, there being a congruity beween them and a future reference or implication attached to ends, while the centre of interest is the self.

Theories of Temperament; An Attempt at Reconciliation. (J. W. Bridges.)

'Temperament' here equals 'total affective makeup.' After considering its relation to instinct, internal secretions, autonomic functions, psychopathic and inherited temperamental types, the author finds indications of the subservience of the whole question to that of autonomic functioning.

The Origin of Laughter. (H. W. McComas.)

Disagreeing with Bergson's view, the author avoids the question of the intellectual factors concerned, points out the close connection of laughter with weeping, and deals with its origin as a social habit, before speech arose, to indicate to other members of the group whatever might be pleasing and worth sharing.

A Theory of Color Vision. (E. Q. Adams.)

This theory is brought into agreement with that of Young-Helmholtz with regard to retinal processes by an assumption as to the light-sensitive substances of the cones, with that of Hering in its psychological implications, except that blue rather than yellow is considered a 'katabolic' colour, and with that of Mrs Ladd-Franklin with regard to development of the colour-sense by assuming a mixture of related substance in the primitive light-sensitive substance.

Mental Reactions to Smell Stimuli. (J. H. Kenneth.)

A short paper dealing with direct and indirect reactions to smell stimuli, varieties of association in the latter, and their use in revealing 'complexes.'

Vol. xxx, No. 2.

The Foundations of Social Psychology. (Knight Dunlap.)

After a friendly criticism of McDougall's theory of instincts, the author suggests that the foundation of social psychology must be laid in a study of the motive forces of 'desires,' of which he tentatively names nine, and which he distinguishes from the Freudian 'wish.' He further suggests the use of data from the theatre on which to base social psychology as a science.

Introspection and Behavior. (R. H. Wheeler.)

Before chemical and physical processes can be regarded as physiological the organic setting must be taken into account. Similarly, says the writer, the behaviourist must take the mind into account before he can call his behaviourist data psychology. Further, he neglects factors which do affect overt behaviour but are uncontrolled by an experimental setting. Introspection needs 'objective' checking, but equally behaviourist data need the help of introspection. Introspection does not differ in kind from other methods of scientific investigation, the distinction between a 'subjective' and an 'objective' method not holding when subjected to a psychological analysis.

An Objective Analysis of Volitional Behavior. (J. R. Kantor.)

Volitional as distinguished from voluntary action—a response to a definite problematic situation—consists of actions within actions, the final reaction being determined by additional stimuli and their responses in addition to the initiating stimulus with attention and perception reactions. Of these additional reactions 'meaning' reactions are more directly connected with the final response than 'auxiliary' reactions.

Criteria of volitional reactions are (1) number and complexity of stimuli, (2) the way they condition the definitive response, (3) general organization of behaviour pattern, giving six types. It is indicated that not all volitional reactions are performative, but some are informational in character, and their development, range and conditions are discussed.

Vol. xxx, No. 3.

Outline of a System of Psychology. (R. H. Wheeler.)

Keeping strictly to a monistic standpoint, and regarding mental processes as differing from motor responses in degree or complexity and not in kind—'covert' and 'overt' behaviour—the writer presents and describes a schematic table showing the mutual relation of these as stages in responses of the organism to the environment. He indicates the way in which behaviour of increasing complexity appears, the extent to which memory is involved, and brings sensation (its one basic content), images, perception, attention and will into connection with 'covert' behaviour.

Biological Influence of Modern Psychology. (A. E. Davies.)

The author considers that the behaviourists derive from the genetic and comparative psychologists rather than from the functionalists as Watson claims. The former better assimilated the biological sciences, and the writer points out the logic of the situation in the increase of the connection between behaviourism and modern physiology and anatomy, with a warning against the preference for still youthful endocrinology over nerve physiology.

The Psychology of Motivation. (F. A. C. Perrin.)

An attempt to harmonize the biological and social aspects of human behaviour. The prestige motive is regarded as basic to social conduct in all its phases and representing the socialized aspect of biological struggle, its different forms being directly traceable to biological needs.

Behavior and Conscious Behavior. (Curt Rosenov.)

The behaviourist posits physical behaviour, says the writer, and still holds to the 'body' side of the philosophical 'mind-body' distinction which presents a problem not germane to psychology. For example, however impossible the study of 'visual sensations,' seeing is an activity open to psychological examination without descriptions as either a mental or physical process. Thus the author describes seeing as the integral working together of partial activities (eye, glands and muscles, nervous system, including automatic habitual movements) without employing any doctrine of parallelism. A distinction is made between conscious and self-conscious behaviour. Study of the emotions should be directed towards them as behaviour and not as states of consciousness, *e.g.* to grieving instead of grief, since we are not aware of organic response in emotion without introspection, although the latter shows that emotion is the awareness of organic response. The additional 'knowing' is an act—a social gesture towards oneself, and similarly, 'meaning' is regarded as an activity. Thus conscious behaviour 'means' the stimulus to which it is addressed and all behaviour which can be objectively observed is conscious, though not necessarily self-conscious, behaviour.

A Behaviorist Account of the Emotions. (E. C. Tolman.)

An article on the same lines as the preceding, and, like it, accepting the James-Lange theory. The emphasis is placed on the impulse phase of emotion, defined as the drive or tendency towards a particular type of behaviour result, of response-as-affecting-stimulus.

The Anti-Instinct Fallacy. (W. R. Wells.)

This, says the writer, is the fallacy of claiming that no action-patterns which are acquired, in the sense of having a developmental history, can at the same time be instincts, *i.e.* inherited action-patterns. If 'inherited' means "transmitted as such in the germ-plasm," then neither are there any inherited somatic traits or characters whatsoever; but 'inherited' actually means "acquired in individual development as a result of germinal factors, or determiners, plus *normal* environmental influences."

No. 4.

The Behavioristic Interpretation of Consciousness. I. (K. S. Lashley.)

Taking the extreme behaviouristic standpoint, the author sets out to "show that the statement 'I am conscious' does not mean anything more than the statement that 'such and such physiological processes are going on within me'." In this first part of his paper he deals with: 1. The present forms and limitations of behaviorism; 2. The evidence for a Mind-Body Problem in (*a*) the distinguishing features of consciousness, (*b*) the subjective definition of Awareness, (*c*) the attributes of the elements of consciousness, (*d*) the problem of the organization of consciousness; 3. The Vitalistic arguments. He finds the adoption of the postulates of the physical sciences also valid for description of mental pheuomena just where introspection fails.

Purposive or Mechanical Psychology. (William McDougall.)

This lecture stresses the impossibility of application to human problems of the distortions involved in Mechanical Psychology, illustrates the point by the history of its development and particularly by an account of the successive phases of Munsterberg's psychological creed. "Behaviorism," says the lecturer, "is not necessarily Mechanical."

Possibility of an Applied Psychology. (W. V. Bingham.)

In furtherance of the above the author draws a sharp distinction between pure psychology, which as a science describes and explains phenomena, and psychotechnology, whose functions are prediction and control.

The Early Development of Hartley's Doctrine of Association. (B. Rand.)

The author shows that the origin of association psychology can be dated back three years beyond Hartley's Observations on Man to a Latin tract 'Conjecturae quaedam, de sensu, motu et idearum generatione,' published by him in 1746, attached to a medical work on Lithontriptics.

The Color Red and the Anger of Cattle. (G. M. Stratton.)

The result of actual experiments on cattle both tame and wild was to disprove the popular belief in the above connection.

Journal of Educational Research.

Vol. v. No. 1. January 1922.

Some Problems arising in the Administration of a Department of Measurements. (Helen Davis.)

A useful discussion of practical problems and the solutions found by the author. The remarks as to test administration and classification are the most generally interesting.

A Diagnostic and Remedial Activity in Supervision. (Bertha M. Rogers and Teresa Baker.)

Deals with the use of tests as a means of judging teaching methods—an aspect of tests that is sure to receive more attention in the immediate future.

Principles of Method Applied in Concrete Situations. (Ruby Minor.)

A suggestive contribution to teaching method, concerned particularly with the use of the problem method in teaching geography.

What Los Angeles is doing with the Results of Testing. (Harlan C. Hines.)

Details the improvements in the grouping and grading of children, and a new marking system, arising from an intelligence survey.

Intelligence in the Primary Grades. (M. Edith Whitcomb.)

The Stanford Revision was used as the basis of promotion from kindergarten to first grade.

Provisions for Individual Differences in High-School Organization and Administration. (W. H. Hughes.)

A summary and analysis of data obtained by a *questionnaire* addressed to four hundred and twenty-five high schools.

Vol. v, No. 2. February 1922.

Arithmetic Ability of Men in the Army and of Children in the Public Schools. (Arthur Kolstad.)

The data of the army examination were used for the adults, and Test 2 of the Alpha Scale was given to 632 children. Some important practical conclusions are arrived at.

A Comparison of the Brightness of Country and City High-School Children. (James H. Hinds.)

The country child appears to be lower in general mentality than the city child, as regards the qualities measured by the Otis group tests.

Lost in Concrete Instances—Many Learners. (Garry C. Myers.)

An interesting pedagogical application of facts elicited by the Beta Scale.

A Method of Commensurating Mental Measurements. (Harry S. Will.)

A demonstration and application of the kental principle to educational measurements. The kental is defined as the unitary value attaching to the one-hundredth subdivision of the standard normal range of ability for any standard test.

Vol. v, No. 3. March 1922.

A Comparative Study of Four Group Scales for the Primary Grades. (V. A. C. Henmon and Ruth Streitz.)

The four scales used in this comparative study were Pressey's Primer Scale, Myers' Mental Measure, Dearborn's Group Test, Series 1, and Haggerty's Delta 1. They are compared with regard to correlations with teachers' estimates, discriminative capacity, character of the distribution curve and ease of administration and scoring.

Educational Measurement as a Key to Individual Instruction and Promotions. (Carleton W. Washburne.)

An application of achievements tests.

Some Uses for Intelligence Tests. (Samuel S. Brooks.)

A very readable demonstration of the practical value of intelligence tests for the purposes of school administration.

The Validity of the Whipple Group Test in the Fourth and Fifth Grades. (Helen Davis.)

It is concluded that the test, originally designed for selecting gifted children in the upper grammar grades, can be successfully employed in the fourth and fifth grades, with certain amplifications in the instructions.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

SECTIONAL MEETINGS.

Educational Section.

June 8, 1925. The Education of the Educationist. By Miss Mary Chadwick.

Medical Section.

June 24, 1925. An Irrelevant Accretion to the Freudian Theory. By Dr Ian D. Suttie.

BRITISH PSYCHOLOGICAL SOCIETY

COUNCIL MEMBERS, 1925.

The Editor regrets that the list of members of the Council of the British Psychological Society published in the last number of this *Journal* was inaccurate. A revised list appears below:

ELECTION	RETIRE	NAME AND ADDRESS
1924	1925	Dr F. Aveling, 8, Thayer Street, W. 1.
1924	1925	Dr P. B. Ballard, 148, Sutton Court Road, Chiswick, W. 4.
1924	1925	Mr F. C. Bartlett, The Psychological Laboratory, Cambridge.
1924	1925	Dr William Brown, 88, Harley Street, W. 1.
1923	1926	Dr C. Burt, 30, Princess Road, Regent's Park, N.W. 1.
1924	1925	Mr E. Bullough, 6, Huntingdon Road, Cambridge.
1924	1925	Mr W. H. A. Dockerill, 75, Ringford Road, E. Putney, S.W. 18.
1922	1925	Miss B. Edgell, 15, Lyon Road, Harrow.
1924	1925	Dr G. Miles, c/o National Institute Industrial Psychology, 329, High Holborn, W.C. 1.
1924	1925	Mr J. C. Flügel, 11, Albert Road, N.W. 1.
1924	1925	Mrs Isaacs, 47, Hills Road, Cambridge.
1923	1926	Dr E. Jones, 81, Harley Street, W. 1.
1924	1925	Dr T. W. Mitchell, Hope Cottage, Hadlow, Kent.
1923	1929	Dr C. S. Myers, 2 L, Portman Mansions, W. 1.
1922	1925	Prof. T. H. Pear, 18, Chatham Road, Withington, Manchester.
1922	1925	Dr Stanford Read, 11, Weymouth Street, W. 1.
1924	1925	Dr J. Rickman, 26, Devonshire Place, W. 1.
1924	1925	Dr R. M. Riggall, 31, Wimpole Street, W. 1.
1923	1926	Prof. C. Spearman, 71, Kensington Gardens Square, W. 2.
1924	1925	Dr W. H. B. Stoddart, Harcourt House, Cavendish Square, W.1.
1924	1925	Miss N. G. R. Taylor, Avery Hill College, Eltham, S.E. 9.
1924	1927	Mr A. W. Wolters, University College, Reading.

VOLUME XVI JANUARY, 1926 PART 3

THE STANDPOINT OF PSYCHOLOGY[1]

BY F. AVELING.

(*From the Psychological Department, University of London, King's College.*)

I. *Present State of Psychology* (pp. 159, 160).
II. *Standpoint of Psychology with regard to the Problem of Knowledge* (pp. 160–162).
III. *The Self as immediately known* (p. 162).
IV. *Examination and Criticism of Miss Calkins's Investigation of Experimental Evidence* (pp. 162–165).
V. *Further Evidence* (pp. 166–169).
VI. *Conclusion and Application to the Standpoint of Psychology* (p. 169).

I. *Present State of Psychology.*

THE state of psychology as a science is admittedly an unsatisfactory one. Not only do psychologists disagree about its definition; they dispute as to its method, and often come to radically contradictory conclusions from the same—or essentially similar—data. Apparently psychology can neither produce a generally acceptable theory nor adduce compelling evidence for its divergent principles. Else, why should psychologists so disagree? There are doubtless many reasons for the patent fact; most of which, in one way or another, and more or less deeply, are rooted in metaphysical—and, more particularly, in epistemological—soil. Apart from the amazingly loose use of terms, which, as has frequently been pointed out, is a scandal in psychology and a reproach to psychologists, the explicit or implied attitude of psychologists towards one or other of the metaphysical systems is enough to account for their disagreement. Psychologists in general are highly sophisticated people. They often come to their science by the avenue of philosophy, bearing their idols of the tribe or of the den, of the market-place or theatre, with them. Or, again, they find urgent problems of psychology cropping up in physical laboratories or in physiological research; and straightway begin to solve them by methods already familiar to them as physicists or physiologists. Doubtless the philosopher has his meaning and use in the world of thought. It would be idle, in these days of applied science, to question the right of the chemist or physiologist or statistician to a foremost place in the sun of knowledge. Indeed, the psychologist himself has titles to consideration in his really valuable contributions to education, psychiatry, industry and commerce. But he has won those titles despite the confusion that disturbs the very foundation of his science considered as a whole.

---

[1] Communicated to Section J, British Association (Toronto, 1924).

I do not wish to prejudice the issue at the outset by suggesting that psychology is a science altogether apart from all the others. Indeed, it has the most intimate and vital connections with philosophy, on the one hand, and the physical and other biological sciences, on the other. All science, though it is for practical purposes mapped out into sections and departments, must in reality be one. But psychology must have a formal subject-matter apart, and a method of dealing with this, else it can lay no claim to being a science at all; and will inevitably come to be regarded—as indeed in some quarters it has come to be regarded—as a study which has abstract fictions for its objects, or a science which has no right to an autonomous existence, since both its object and its method are those of general biology.

Now, psychology certainly cannot be helped in this matter by the philosopher if he comes to a study of its facts with foregone conclusions; nor can it be advantaged by the physicist or physiologist if he insists upon applying his categories too rigidly to the data with which psychology provides him. How is a physical occurrence in a nerve circuit to explain a thought or fear or aspiration; any more than an idea, or monad, or the unconscious, explain the occurrences of our conscious lives? And if we turn from the philosopher and man of science professed to the plain man, who just takes things for granted, he neither formulates nor answers the question which all of us ask; but will just dumbly persist in seeing an objective world over against him—a real and resistant world which will not always fit in with his schemes, nor satisfy his desires, nor (above all) explain itself to him any more than he, if he thinks, can explain himself.

II. *Standpoint of Psychology with regard to the Problem of Knowledge.*

The problem of the origin and worth of knowledge, the solution of which is *presupposed* by the objective sciences, whose discussion has filled so many volumes of philosophy, should give psychologists pause; but we should not shirk it, nor allow it altogether to baffle us. For surely this, if anything, lies in our own especial psychological territory.

And in an examination of it may be found an indication of that for which we should search in psychology. We can hope to discover it nowhere else; but, *if we can find within the mind itself any ultimate real*, we shall have gone far both towards a solution of the epistemic problem and towards providing a solid foundation for psychology, by means of which a common standpoint in the science may be made possible. Nevertheless this problem, if it is to be investigated from the point of view of psychology, must be solved by facts, and not by mere appeal to theory.

We may take it, for the purpose of this paper, that the formal subject-matter of psychology is Mind, or Consciousness, which is generally regarded as one form of response which the living individual makes to environment. It is true that both Mind and Consciousness are conceived of in very different ways; but it is on the whole agreed that we have some sort of direct access to it, and can, whatever our concept of it may be, know *it* in direct awareness.

Now, psychology might proceed in its scientific investigations as do the objective sciences. That is to say, it might take the solution of the problem for granted, and study its subject-matter, as they do, by inspection, experiment and, where possible, measurement; adding reflection to these as its own particular contribution to the instruments by which knowledge is acquired. This, as a matter of fact, *is* the plan followed by the majority of psychologists. But the objection to it is that, whereas the subject-matter of the other sciences consists of things known, indeed, but studied by them as if they were entirely independent of knowledge; the primary subject-matter of psychology consists, among other (but in this respect precisely similar) things, in the very processes of knowing themselves and in objects precisely as known. Even if Consciousness or Mind were in reality to be no more than a web of sensory processes, that web would itself be woven either of the strands of cognitions or—what is essentially similar in the sense of being subjective—of affections and conations.

But a better plan for psychology is, I suggest, that nothing but what is actually given as evident should be supposed; that evidence should only be accepted in the most radically critical way, either as a primitive and unanalysable datum, or as one to be explained by some reasonable hypothesis.

I am aware that in what I have been saying I have been forced by the necessities of language apparently to beg the question at issue. But we must work with what tools of expression we possess; and nowhere have I seen the problem touched upon without this seeming *petitio principii*. Nevertheless, as I shall hope to show, even the inevitable forms of language may be justified in this matter; and that with a better reason than the arguments—if indeed they are arguments—*cogito, ergo sum* or *quod si fallor sum* of the philosophers. At least, in adopting the plan I suggest, we shall be sure that such knowledge as we admit will be as valid as *any possible knowledge can be,* and that the reality it mediates is as real as any known or knowable real can be.

My thesis, then, is that some reality, as such, is directly given in experience; that not all reality is phenomenal as distinct from noumenal; that what appears, in so far as it appears, *may* appear as it really is. There is an ambiguity here in the word phenomenal itself due mainly to the contrasted use of the terms appearance and reality in modern philosophy—as if no reality *could* appear as it actually is. To this ambiguity I can only draw attention in passing, with the warning that I use the term only for lack of a better one. The facts should not be prejudiced by hypotheses attaching to words.

III. *The Self as immediately known.*

The reality which I hope to show as an immediate object of awareness certainly 'appears.' It is, I submit, the real and profound Self. This thesis is neither an entirely new nor original one. It is, indeed, implied in the *cogito* of Descartes, in the *quod si fallor* of St Augustine; for what in either case appears to be the conclusion of an enthymeme, is in reality only a part, or *explication*, of the statement of fact contained in the pseudo premiss. Otherwise, the reasoning, in both cases, would be wholly inconclusive. Both Descartes and St Augustine, we may believe, were good enough psychologists to realize what was implied in their formulas.

But in a sense the thesis, at any rate in the manner of its treatment, *is* new, and of late has attracted considerable attention; for the problem has been raised afresh within the province of experimental and scientific psychology. And it is from this point of view that I propose to examine it in the present paper.

IV. *Examination and Criticism of Miss Calkins's Investigation of Experimental Evidence.*

A great deal has already been done by Miss Calkins in the direction of clearing the ground and investigating the experimental evidence for and against an immediate awareness of the Self. In her books and contributions to periodicals—in particular, in her controversy with Prof. Raymond Wheeler—she has reviewed and estimated the introspective reports of a number of experimental researches in psychology; and she has met the criticisms urged by the spokesmen of the opposing camp (Wheeler and Curtis).

Though there appears to me to be considerable confusion in the minds of those who engaged in this controversy—and especially in the matter of the gravely ambiguous use of the term 'Self' as denoting both that which is immediately experienced as an existent and that which is conceived of as a nature, it will be useful to present a very brief account of Miss Calkins's analysis before bringing forward the evidence of my own observations in the matter.

Miss Calkins[1] regards the Self as indefinable, but not therefore elemental and indescribable; and she makes her own the distinction that its characters are real properties and not logical differences. I understand that this is meant to emphasize the distinction between the concept of the Self and the intuited Self. She then proceeds to enumerate four characters of the Self as actually experienced: viz. self-identity, uniqueness, relatedness as ground to its experiences, multimodal relatedness to its environment; and she concludes that "the psychological description of the Self needs to be clarified, widened and enriched by the efforts of all psychologists working in all branches of the science."

Calkins next examines methods; and selects as the best that of systematic introspection, by trained introspectors, of experimentally controlled mental operations, in the interest of some theoretical problem. In this she maintains, the Self, if it exists, should be found; and unless it be discovered by this method, it can have no status in experimental psychology.

The researches she selects for examination, as disclosing the presence of the Self, are those of Dürr[2], Katzaroff[3], Michotte and Prüm[4], Ach[5], and Boyd Barrett[6]; and in all these she finds strong introspective evidence in favour of her main contention that the Self is immediately given in awareness.

In a further paragraph Calkins cites a number of psychologists (not necessarily experimental) who hold either (1) that we are conscious of

---

[1] "The Self in Scientific Psychology," *Am. J. Psy.* 1915, 495.

[2] Die Lehre von der Aufmerksamkeit, Leipzig, 1907.

[3] "Contribution à l'étude de la récognition," *Archives de Psych.* 1911, XI.

[4] Étude expérimentale sur le choix volontaire, Louvain, 1910.

[5] Über den Willensakt und das Temperament, Leipzig, 1910.

[6] Motive-Force and Motivation-Tracks, London, 1911.

Self in all conscious acts, or (2) that we are conscious of Self only in certain kinds of experience, as in the affective states. She might have added also those who, on the ground of introspective evidence, cautiously "adopt the conservative attitude of attributing 'the Self' to direct experiential apprehension," as well as those who need the Self (or Mind, Soul or Subject) as a necessary and acceptable hypothesis in psychology.

Lastly, Calkins attempts to explain the failure of those who do not find the Self on introspection; and the fact that they question and rather superciliously criticize the introspections of the psychologists who do so find it, even when their own accounts of mental operations "are full of the terms of Self-psychology." She gives four reasons for this failure: (1) one is always conscious of Self, and therefore fails to attend to it; (2) systematic introspection has been principally concerned with sensory and thought processes of an objective character; and it is not likely that the unemphasized consciousness of Self would be reported here; (3) instructions given to Subjects are specific, and seldom, if ever, mention the Self; Subjects will therefore tend to report only what they were told to observe; (4) Subjects frequently have arbitrary preconceptions as to the scope and character of introspection. Sometimes (as at the Cornell Laboratory) these are limited by definite instruction to structural elements or mental functions.

If I may anticipate my own formal contribution to this subject, I should wish to add to these reasons, or to present them in a slightly modified form. (1) In the first place, the consideration that the Self is always given when one is conscious and that we therefore fail to attend to it, is paralleled by the continuous general coenaesthesia, of which we are rarely focally conscious. In the second place, by supposition we can have no possible negative instance in the matter of the Self; and hence are liable to overlook it. The method of concomitant variations was devised to meet precisely such a case, in which we are unable to eliminate a permanent cause; and there is evidence, objectively controlled, of concomitant variations in respect of the reported presence of the Self[1]. (2) Of the greatest introspective importance in this regard is the fact of the narrowing or shrinking of the field of awareness to th precise operation in hand at the moment. This is very striking in all introspective work; and in my own observations, mainly on processes of conception and of choice, it was markedly so. During the period of deliberation in choice, for example, rarely were any processes focal save

[1] Cf. Wells, "A Note on the Psychological Significance of the Psycho-galvanic Reaction," this *Journal*, Jan. 1924.

cognitive ones connected with the alternatives: bodily strains, kinaesthetic images, imageless thought, and the like, sometimes coloured by affective factors. When the Self becomes momentarily focal all these processes cease. But, in any case, the Self tends to be masked by other processes of a cognitive or affective character; and this according to the nature of the instruction given to the Subject. (3) Moreover, the bodily strains, images, etc., are prominent not only because one is actively attending to them but also because they are relatively full of content. The Self—at any rate in the experiments to which I refer—is, when focal, as we shall see, relatively empty of content, very fleeting, and readily displaced from the focus by objective processes especially of a sensorial character. It can, however, quite well be distinguished from these.

There is one other point to which I wish to refer before passing on to a cursory examination of Miss Calkins's characterization of the immediately experienced Self. We are told that it is relatively persistent. This, I believe, can be introspectively supported, and accounted for by the non-punctual nature of the specious present. There is a before and after in the mental *now*, in which the Self manifests itself in introspection. And it is always the same Self; though it may in this time of presence manifest itself in different ways. There is much introspective evidence to be adduced in support of this statement.

With regard to uniqueness, I am not clear or certain what is meant by Miss Calkins; but if it is a character of otherness from the objective contents and even from the static and dynamic states, I think I am prepared to admit it, but with the *caveat* that the 'unique' Self may not always present itself with the same formal property.

But Miss Calkins includes complexity as a character of the Self as experienced. Here I am in disagreement. Surely the complex Self is the concept, built up of a number of notions or notes severally and disjunctively experienced in the Self of which we are immediately aware. This is, as has been said, almost totally devoid of content, and is experienced in the most simple form. There appears to me to be a similar confusion of the immediately intuited Self and the concept and intuition of Self in the multimodal relatedness which is also designated as a character. That Self may be introspected in relation I am disposed to admit; that it can be intuited in multimodal relatedness *cannot*, I think, be in any way supported by the introspective evidence. In the main, however, and with these reservations, I find myself in agreement with Miss Calkins.

V. *Further Evidence.*

The introspective evidence which I have to contribute in support of the immediate intuition of Self is derived principally from four sources: my own published research on the Consciousness of the Universal and the Individual together with my further work, *Contributions expérimentales à une théorie du processus cognitif*; and two as yet unpublished researches, the one on choice, the other on strength of will, carried out by advanced students in my laboratory, Dr H. M. Wells[1], and the Rev. Dr R. C. McCarthy, respectively. I wish to acknowledge my indebtedness to them for the introspective data with which they have furnished me.

The first-mentioned research brings out very clearly the distinction of the Self to be drawn between concepts, percepts and that part of the percept which is actually and directly given (we may for our present purpose neglect reproductions), and, if but for this alone, is of importance in this question of the awareness of Self. For it is indisputable, as I shall hope to show, that we may have all three forms of knowledge of it; and that, of them, the third may be immediate. But a further distinction may be drawn within the third form of knowledge itself. The part actually given may be objective and sensorial, as when I may be said to perceive myself when seeing my hand; or it may be purely subjective, as when I am immediately aware in introspection of Self-judging, -choosing, -universalizing, and the like. It is this last case—and this case only—in which we are primarily interested at present. Now, though my own research was planned to investigate our consciousness of universals and individuals in order to elaborate a theory of thought—and *pro tanto* tended to exclude the reporting of Self-components—there is much evidence, even in the protocols published in my book which were selected to bring out quite other points than this, of thoughts of Self, feelings of Self (evidently, in the context, non-sensorial), memories of Self doing something, attempts of Self to make words function as universals, accepting of meanings and refusal to use meanings, and the like. And these reported processes can have no possible signification unless they refer in some way to an immediate awareness of the Self so feeling, thinking or acting. They were reported by all my six Subjects and by myself, especially in connection with universals; and I have no hesitation in classing them with the results of Ach and Michotte and Prüm, as well as with the various kinds of introspections derived from the researches of my own students. I distinguish, then, clearly between concept of Self, percept of Self, and intuited Self; and conclude that we may and do *experience* all three. But this conclusion is here based rather on inference from the processes reported; for there was no case in my research in which a Subject clearly stated the occurrence of an intuition of Self. This fact, however, should not astonish us; for it is well known that the most patient and repeated examination of the processes of consciousness is necessary in order to determine and discriminate them clearly. Little by little in introspection a phenomenon comes to be observed and characterized. At first the Subjects know "that there is something present more than they have reported." Then they report it in tentative terms, thus distinguishing it from other processes observed; and in the end they may find a term more or less appropriate to designate it. This happened in the investigation of Michotte and Prüm. It happened also in that of Miss Wells. Indeed, the occurrence must be almost a commonplace to anyone who has practised much introspection. When we remember what has been said already as to the elusive and fleeting focal intuition of the Self, and its tendency to be *masked* by objective, and especially by sensorial, processes, not over much weight will be attached to the absence of a direct report of it in the protocols of a research upon conception.

---

[1] This work is now in the press.

The researches on choice and on strength of will to which I have referred were of a nature to bring out more distinctly the immediate intuition of the Self. The first was planned—on lines somewhat similar to those of Boyd Barrett's[1] investigation—to study the process of choosing between pleasant, unpleasant and indifferent tastes of various kinds; and in it time and breathing records, as well as records of the psycho-galvanic reaction, were taken. The disturbances in the breathing curves and the deflections of the galvanometer were found to synchronize with the critical periods in the choice-reactions; and Subjects were actually able to report introspectively on the amount of Self-action experienced in acts of decision, etc. These estimates were made entirely subjectively and were found to correspond in a quite remarkable way with the percentage decrease in resistance objectively registered by the galvanometer.

The second research was a modification of that of Ach[2]. In it the Subjects were required to find a rhyme to one nonsense syllable of learned nonsense words of two syllables, to invert the order of its letters, to change its vowel, to make a choice between either of two of these operations, etc. The syllables were presented in different colours which symbolized the operations to be performed, thus securing that the whole process to be observed should fall within the introspective period between the exposition of the stimulus and the reaction. It was found necessary during the course of the experiments to interpose *unknown*, but similarly coloured, nonsense syllables *with* the learned ones. All the stimuli-syllables were shown in a tachistoscope; and time, breathing and psychogalvanic curves were registered. Neither research was directly concerned with the Self; but in both observations in its regard began to be made spontaneously by the Subjects; and the introspective accounts seemed to be of sufficient interest to warrant some special attention being paid to the matter during the further course of the experiments. Accordingly, once a Subject had reported its occurrence, or the occurrence of something judged by the Experimenter to be reasonably like it, he was told to try to characterize his experience more fully. It was not of course suggested that he should look for the Self, but only that he should attempt to observe as accurately as possible what he had already experienced and reported.

---

[1] *Loc. cit.*

[2] *Loc. cit.*

By this means a very considerable amount of introspective material was obtained from each research.

It was found possible roughly to separate the reports, in so far as they make mention of Self, into four groups. The first contains reports of "Reference to Self."

The second group of reports embraces those cases in which the Self is given as subject of feelings, doubts, and the like.

The third group comprises cases of awareness of Self in consciousness of action. This term is adopted from Michotte and Prüm. When the process is reported the Subject appears to be immediately aware of himself as the experiencer, actor, sufferer, etc.

Consciousness of action is also shown as a subjective process, fleeting and difficult to catch, the more vivid cognitional elements of consciousness tending to mask it.

The fourth group is made up of those cases in which awareness of Self is directly reported.

Though it was found possible to some extent to classify the reports of Subjects as above, the reports shade off one into the other in such a way that many of them might have been included in more than one group. The historical development of these reports is interesting, and throws light upon the character of the fact observed. Subjects began, as a rule, by discriminating the mental consciousness of action from the 'physiological' tensions and activity. Later came the references to Self; but it was not clear at this point whether the Self referred to was not merely the concept. A theoretical question arises here as to how reference could be made to the concept, or even how this could have come to be formed at all, in the absence of some nuclear direct experience of the real Self. The Subjects, however, were not troubled by this difficulty, which did not occur to them until afterwards; and went on to characterize their consciousness more explicitly in terms of Self as subject of feelings, etc., and in other unambiguous statements of immediate Self-experience.

The main objection that has been urged in the past against the thesis which these introspections support is that it abandons the empirical standpoint of scientific psychology for a metaphysical one. But the objection itself is based on quite arbitrary assumptions; first, that all that of which we may become aware must be in itself other than it is in our awareness of it—that empiric and metaphysical must necessarily be exclusive; and, secondly, that, having defined consciousness as the sum of its (possible) contents and states, we should be obliged to attach the same objective—ontological—value to all the data we may get from an examination of it. Now this objection appears to me to be in itself of an entirely metaphysical character, to close the door to impartial investigation of consciousness and to be contradicted by the facts of experimental psychology. There must be no metaphysical fetters, in the shape of postulates of method, for psychology.

VI. *Conclusion and Application to the Standpoint of Psychology.*

The Self, then, it may be maintained, is as such an immediate datum of awareness. How can we describe it; and what is its nature? As to description, it is clear from the Subjects' protocols that it can only be described, each time it is experienced, as a bare entity acting in some way. There is no hint of awareness whatever as to what the entity may be. It is just an existent acting; no more. But when we come to examine the concept which we certainly have of the Self, we find this quite another matter; for it is a notion that is particularly full of content, and it has none of the peculiar and intimate character of actual action which is given in the intuition. On dissection, it is found to be made up of a large number of notions as to how the Self *can* act, since it has so acted; or how it *might* act, since it has acted in more or less similar ways. I do not propose to consider the nature or genesis of the concept here; but it is of the greatest importance to distinguish it clearly from the immediately experienced Self of intuition. If, as I have maintained, we have an immediate awareness of the real and profound Self in experience, this fact has far-reaching consequences in psychology. Consciousness may still be methodically treated objectively as the sum of its contents and states, but with the *caveat* that this treatment is merely methodological, and not exhaustive. Or it may be treated from the point of view of act and content, with the emphasis on act; in which case the act or functional response of the organism in the different modes of consciousness, will be recognized as that of the profound Self of intuition; just as behaviour is recognized as the functional response of the organism to stimulation. But the former with more right than the latter; since it is possible, and even necessary, to regard the organism, so far as it is known, as a mere phenomenon.

In conclusion, immediate awareness of the real Self would justify the forms of language which even psychologists must use, and save us from the barbarisms of attempting to express, in purely objective and impersonal terms, the processes of mind which we observe.

CONVERGING LINES IN CONTEMPORARY PSYCHOLOGY

By MARY WHITON CALKINS.

(*Wellesley College, Wellesley, Mass., U.S.A.*)

Many contemporary systems of psychology unite (1) *in a protest against the atomistic conception of psychology as a study of independent processes or contents, and* (2) *in the conception of psychology as the study of a unified being in varying relation to its environment* (p. 171).

I. *The first of these systems is behaviourism, which studies the integrated living organism and is contrasted with all the others by "ruling out consciousness"* (p. 172).

II. (i) *The second is* Gestalt-psychologie (p. 173).

  (ii) *The third is psychophysical personalism, the study of the living and sometimes conscious organism* (pp. 173–175).

  (iii) *The fourth is strictly psychological personalism, which studies the essentially conscious self* (pp. 175–177).

  (iv) *The fifth is the psychology of the Unconscious* (pp. 177–178).

THE modest aim of this paper is to set forth clearly a fundamental similarity between certain contemporary systems of psychology which at many other points diverge sharply from each other. Negatively stated, this agreement consists in the common protest against atomism in psychology, that is, against the conception of psychology as a study of separate psychic 'processes,' or 'states,' or 'contents.' The positive conception shared by these critics of the atomistic theory is that of psychology as the study primarily not of processes, states, or contents, but of beings or entities; as the study, in a word, of organisms, persons, or selves. An organism is understood as a whole which does not consist in the elements which it includes; which is neither an aggregate of elements, nor yet one favoured element of an aggregate[1]; which is, rather, an integrated, individuated, single being including many members or parts subordinated to itself.

The conception summarized in the preceding sentence is central to many significant systems of present-day psychology.

---

[1] W. Stern, *Die Menschliche Persönlichkeit*, p. 5.

I. First of these may be mentioned behaviourism, which has from the very start decried the over-abstractness of the exclusively analytic study of so-called psychic contents, sensations, affections and the rest[1]. In the view of its best-known protagonist, behaviouristic psychology studies the reactions which the "totally integrated individual makes to its environment[2]," or deals with the "whole organism" in its relation to "its environment as a whole[3]." And though, as its keener critics have already discovered, behaviourism in actual practice is often in its own fashion atomistic, treating of separate reactions rather than of the organism as a whole[4], there is yet no question either of the theoretical position of behaviourism or of its frequent applications of this basal conception of the reacting organism[5].

II. In addition to behaviourism, there are four types of anti-atomistic psychology: these are, first, *Gestalt-psychologie*; second, personalistic psychology of two distinct sorts; and finally (paradoxical as it must sound), the psychology of the Unconscious, in most forms of it. All three, despite the fundamental likeness which this paper stresses, must be sharply distinguished from behaviourism in that they treat the units of psychology as (wholly or in part) conscious beings; whereas behaviourism, in the only sense in which it is a distinct system, rules out consciousness, "can get along without" perception, attention, emotion and the like[6]. For this reason, the systems just enumerated are here grouped together and set off against behaviourism. The pages which follow summarize, in greater or in less detail, the relevant doctrines of three of these systems which concern themselves with integrated beings conceived as characteristically and often, or else as wholly and always, conscious. To the psychology of the Unconscious only brief reference will be made.

---

[1] Cf. John B. Watson, "Psychology as the Behaviorist views it," *Psychological Review*, 1913, xx, 163: "The time seems to have come when psychology must discard all reference to consciousness....We have become enmeshed in speculative questions concerning the elements of mind, the nature of conscious content...."

[2] John B. Watson, *Psychology from the Standpoint of a Behaviorist*, p. 13. Cf. pp. viii, 12, 48, 193, 209–end.

[3] *Ibid.* p. 20. Cf. A. P. Weiss, "Behavior and the Central Nervous System," *Psychological Review*, 1922, xxix: "Behaviorism is the science that studies the origin and development of those bodily movements of the organism which establish its social status" (p. 330 f.).

[4] Cf. the criticism of K. Koffka in "Introspection and the Method of Psychology," this *Journal*, 1924, xv, 152: "If we would observe behaviour, we must [the behaviourists say] observe it bit by bit."

[5] Cf. the chapters on "Hereditary Modes of Response" in Watson's *Psychology from the Standpoint of a Behaviorist.*

[6] *Psychology from the Standpoint of a Behaviorist*, pp. viii, 325 *et al.* The term 'behaviorism' is popularly used to designate also systems which concern themselves largely with bodily reactions, though they also deal with introspective data. But nothing distinguishes behaviorism if thus conceived, from many contemporary introspective systems.

(i) To begin with the youngest claimant to a place among systems of psychology: the *Gestalt*-psychology of Wertheimer, Koffka, and Köhler: no one who knows it, however inadequately, needs to be told that it is hotly opposed to the atomistic systems, whether psychological or physiological. Its central doctrine is the conception of percepts and thoughts on the one hand, and of physical objects on the other, as unified wholes of subordinate parts and not as mechanically added sums of independent units. So Wertheimer inveighs against the 'bundle hypothesis' in psychology; and Koffka criticizes the traditional doctrine that "all consciousness consists of a finite number of real, separable... elements, each element corresponding to a definite stimulus[1]."

To this 'bundle hypothesis' the *Gestalt*-psychologist opposes the conception of a 'structure' (*Gestalt*), an "undivided, articulated whole," not "a purely summative group of simultaneous elemental events[2]."

In the more or less shadowy background of this doctrine of percepts and judgments, as themselves 'wholes' which are not mere 'sums,' is the less strongly emphasized conception of the 'experiencing individual[3],' or 'conscious organism[4],' as Koffka variously calls it. Consciousness he describes as one of the reactions of the organism on its environment; and he says expressly that "the organism knows itself." It must however be admitted that, in spite of these unambiguous statements, and in spite, also, of the fact that an experiencing individual is obviously itself a *Gestalt* or structure, an organized whole of co-operating parts, the *Gestalt*-psychologists make little systematic or concrete use of this conception, for the most part discussing percepts and judgments rather than perceiver or judger. We must turn then to the explicitly personalistic forms of psychology[5] for the discussion of this aspect of anti-atomism, its doctrine of the 'experiencing individual.'

(ii) Personalistic psychology significantly differs from *Gestalt-psycho-*

---

[1] "Perception: An Introduction to *Gestalt-Theorie*," *Psychological Bulletin*, 1922, XIX, 533.

[2] Cf. W. Köhler, *Die Physische Gestalten*, p. xviii.

[3] K. Koffka, *The Growth of the Mind*, p. 16.

[4] Koffka, "Introspection as the Method of Psychology," this *Journal*, 1924, xv, 153.

[5] The term 'personalistic psychology' is used throughout this paper with the wide meaning which Stern gives to it and not as a synonym for what the writer of this paper calls 'self-psychology.' As will appear, self-psychology is one form only of personalistic psychology in this larger sense of the term.

logie precisely in its emphasis on just this conception, so lightly touched by the *Gestalt*-theorists, of the conscious or experiencing individual. In the view of the personalist, this is indeed the basal concept of psychology. In Stern's phrase, conscious processes and processes "belong to persons (*hören Personen an*)[1]." The unit of psychology, accordingly, its basal topic of study, is neither process nor content nor reaction, but the being which changes, which reacts, and which experiences (or 'has' contents).

Among themselves, it must next be noted, these personalistic psychologists divide sharply into two groups, the psychophysical and the narrowly, or strictly, psychological personalists. By the former group, which includes besides Stern, William McDougall[2], Angell[3], and (among biologists) Jennings[4], the experiencing individual is the conscious organism, the organism whose consciousness is simply one reaction among others, though a supremely important and distinguishing reaction, upon the environment. From this point of view, it is obvious, a person is not an invariably and essentially conscious being; it is indeed a living being more primitively and more fundamentally than a conscious being. In Stern's phrase, "Aus dem Leben erwachst ihr Erleben[5]." The basal character of the person, thus conceived, is accordingly its individual and 'structural' wholeness. So Stern describes the person as "an existent which in spite of the plurality of its parts, constitutes a real and distinctive unity[6]."

Personalistic psychologists of the second, the more strictly psychological, type include Ward[7], Rehmke[8] and Mitchell[9]. These writers, and many others of the same way of thinking[1], conceive psychology as study of essentially conscious beings—selves, subjects, or persons. In the view of some of these writers, persons or selves or subjects are not 'sometimes conscious,' but 'always conscious'; and, in the view of all of them, the distinctive relation of person to environment simply consists in consciousness. Accordingly, personalists of this type conceive the psychologist's topic of study not as psychophysical organism, sensitive, mobile, and sometimes conscious, but as conscious self which has a sensitive and motile body[2].

---

[1] *Die Menschliche Persönlichkeit*, p. 123. A confirmation of this observation, from an unexpected source, is Bertrand Russell's "Thoughts and feelings do not occur when there is no self for them to belong to" (*Our Knowledge of the External World as a Field for Scientific Method in Philosophy*, Lecture III, p. 75).

[2] *Psychology, the Study of Behaviour*, 1905; *An Introduction to Social Psychology*, first edition, 1909; *Outline of Psychology*, 1923.

[3] "The Province of Functional Psychology," *Psychological Review*, 1907, xiv, 61 ff.

[4] H. S. Jennings, "Mechanism and Vitalism," *Philosophical Review*, 1918, xxvii. Cf. p. 594.

[5] *Op. cit.* p. 234. Cf. p. 223.

[6] W. Stern, *op. cit.* pp. 4 f. To the group of psychophysical personalists, thus defined, really belong the exponents of the theory sometimes called modified behaviourism, even though they might disclaim the affiliation. Their view is well represented by H. C. Warren, who describes psychology as the "science which deals with the mutual interrelation between an organism and its environment," but who also protests against ruling out "the data of consciousness" (*Human Psychology*, pp. 13, 431 note. Cf. the writer's "The Truly Psychological Behaviorism," *Psychological Review*, 1921, xx, 8 ff.).

[7] *Encycl. Britannica*, 9th and 11th editions, article on Psychology; "On the Definition of Psychology," this *Journal*, 1904, I. 1 ff.; *Psychological Principles*, chap. II.

[8] J. Rehmke, *Lehrbuch der Allgemeinen Psychologie*, 1ter Teil.

[9] W. Mitchell, *Structure and Growth of the Mind*, 1907, Lecture I.

It is perfectly obvious that these two forms of personalism agree in the basal conception of psychology as study, not of independent bits or items, but of integrated beings. And the more closely the two systems are studied, the more evident becomes their agreement, also, in concrete detail. This is readily illustrated from the pages of personalistic writers. One may, for instance, select, as example of the psychophysical conception, Stern's account of the nature of the person. The characters which follow, in his view, from the *unitas multiplex*, the fundamental one-ness of parts which constitutes a person, are (1) its permanence in change (*einheitliches Sein braucht nicht starr zu sein*); (2) its purposefulness, or effectiveness (*einheitliche, zielstrebige Selbstthätigkeit*)[3]; and (3) its individuality. ("In spite of all agreement...there remains always a last unique quality, *ein letztes Ureigenstes*, by which every person is contrasted with every other.") And finally, though Stern does not expressly include it in this list of basal characters, there is the relatedness of the self to its environment. "The personal individual is," he says, "bound by double links to the world, temporally to the succession of its race, spatially to the structure of its environment[4]."

(iii) With this psychophysicist's description of the person, the writer of this paper ventures to compare her own account written from the narrowly, or strictly, psychological point of view. According to this statement, the 'conscious self' is immediately experienced as possessed of at least four fundamental characters. I experience myself "as

---

[1] Cf. the writer's "The Self in Scientific Psychology," *American Journal of Psychology*, 1915, XXVI, 519, 505 ff., for further references.

[2] Cf. the writer's *A First Book in Psychology*, p. 277, and a paper in the *Journal of Philosophy*, 1908, V, 13.

[3] W. Stern, *op. cit.* pp. 5–7.

[4] *Op. cit.* chap. III, p. 95. Stern calls this the doctrine of the 'convergence' of person and environment. Cf. also McDougall's detailed studies of the reactions of animal and human subjects to their environment (*Psychology, the Study of Behaviour*, pp. 19 ff.; *Introduction to Social Psychology*, *passim*; *Outline of Psychology*, chaps. II–IV).

(1) relatively persistent—in other words, I am in some sense the same as my childhood self; as (2) complex..., as (3) a unique, an irreplaceable self —I am closely like father, brother, or friend, but I am after all only myself: there is only one of me. I experience myself (4) as related to (or conscious of) objects either personal or impersonal[1]."

The essential likeness of these descriptions, the psychophysicist's and the self-psychologist's, is perfectly evident, and becomes more striking when one realizes that they were written more than a decade apart in entire independence of each other. Personalists of both sorts, it is evident, in describing the person or self, stress not only the character common to all anti-atomistic conceptions, the fundamental oneness of many parts, the complex unity, but in addition the uniqueness of the self, its persistence in change, and its relatedness to environment. In the words of Ward, earliest and most distinguished of the writers of this type, "the standpoint of psychology is that of the living subject in intercourse with his special environment[2]." Psychophysical and strict personalists even go so far as to agree in distinguishing as ground forms of this relation the 'receptive' from the 'initiating,' or 'active,' attitude of the self[3]. It is true, of course, that the psychophysical personalist conceives consciousness as one only of the forms of this relatedness, but on the crucial point, that psychology must concern itself with the relation of 'person' to environment, the two forms of personalistic psychology are in emphatic accord.

A final and parenthetical comment must be made on the most important difference between contemporary psychophysical personalism and full-fledged self-psychology. Psychophysical personalists teach that a purposiveness, unconscious as well as conscious, is the distinguishing mark, the most central character as Stern calls it, of personality[4]. "The person," Stern continues, "is end for itself (*sich selber Zweck*)[5]." This inner purposiveness of the conscious being, so strongly emphasized by the psychophysicists, is by no means, they insist, to be confused with conscious purpose, or volition[1]. Rather, volition is one form only of a psychophysically neutral purposiveness which includes also adaptiveness, organic selection, and anticipatory reaction to future stimuli. Protective coloration is, for example, purposive but is certainly not purposed or chosen by the protectively coloured animal. The assimilative selection by the stomach of certain constituents of food is another instance of unpurposed, and in this case even unconscious, purposiveness. Finally, so-called anticipatory reactions, the playful fights of young kittens, for example, which prepare them for genuine adult combat—these also are instances of unpurposed but purposive reactions[2].

---

[1] M. W. Calkins, *A First Book of Psychology* (first edition, 1910), p. 3. Cf. the first statement of these characters in *An Introduction to Psychology*, first edition, 1901. Note also the writer's later express inclusion of growth, or change, along with persistence ("The Truly Psychological Behaviorism," *Psychological Review*, 1921, XXVIII, 14 f.).

[2] "On the Definition of Psychology," this *Journal*, 1904, I. 1 ff.

[3] Cf. Stern, *op. cit.* chap. IV, pp. 124 f.; and M. W. Calkins, *An Introduction to Psychology*, first edition, 1901, 154, 306 ff.; *A First Book in Psychology*, fourth edition, 1914, 3, 11 f., 226.

[4] *Op. cit.* p. 17.

[5] *Op. cit.* p. 19.

The 'narrowly psychological' personalist rejects this conception of unconscious purposiveness. He fully admits the utility, the adaptation, the value of a vast number of animal and human acts performed by the particular animal in question without end or design, and often unconsciously. He holds, however, in the first place, that these acts are known as purposive only because they do carry out some one's purpose—that of an appraising on-looker if not that of a controlling mind. And purposiveness, when thus accurately described, seems to the self-psychologist, in the second place, a biological or metaphysical and not a psychological character, since psychology primarily deals with the immediately experiencing individual. Thus, the fact that a person's acts are purposed or valued by some other person is, in the self-psychologist's view, only secondarily a factor of psychological description. For, he repeats, a psychologically purposive self, or person, would be in some sense conscious of purpose; but it is commonly agreed that persons or selves are *not* at all times conscious of purpose; and therefore psychology should not count purposiveness as a constant and essential character of the person[3].

(iv) The adequate discussion of the personalistic aspect of the so-called

---

[1] Cf. McDougall's description of purposive movements as "those which present the appearance of seeking a goal" (*Outline of Psychology*, p. 53). More often, however, McDougall describes purposive movements as those which "are made for the sake of attaining their natural end," and states explicitly that "this end is more or less clearly anticipated or foreseen" (p. 47). Admitting that he "cannot provide any conclusive proof of this ascription of purposive or mental quality to the action of any animal" he none the less holds that the goal is foreseen "however vaguely or incompletely."

[2] Cf. W. Stern, *Die Menschliche Persönlichkeit*, pp. 134, 142; W. McDougall, *Outline of Psychology*, pp. 47 ff., 195 ff.; R. F. A. Hoernlé, *Philosophical Review*, 1918, xxvii, 594.

[3] Cf. two papers by the writer: "The Personalistic Conception of Nature," *Philosophical Review*, 1919, pp. 119–122; and "Purposing Self *versus* Potent Soul," *Journal of Philosophy*, 1917, xiv, 197 ff. The alleged unconsciously purposive disposition or act or organism is really so described as it is observed to be 'means' to an end apprehended by the observer.

New Psychology, or psychology of the Unconscious, would carry us too far afield; it would involve interpretation of too many obscure and conflicting and often self-contradictory conceptions. Accordingly, the briefest mention will be made of the anti-atomistic trend discernible in the psychologies of the Unconscious. It must indeed be admitted at the outset that systems of this type have re-enforced in the popular mind one emphatically atomistic conception, that of the independently existing complex, "split off from the rest of consciousness" and exerting a mysterious power of its own[1]. But complexes are often conceived in personalistic fashion, not as independent psychic beings but as affectively toned, impulsive tendencies, or dispositions, of some underlying entity. For example, along with Hart's atomistic accounts of the "systems of ideas split off from the rest of consciousness," we find in his pages personalistic descriptions in which the title rôle is played by the self, mind or person to which desires, impulses and ideas are subordinate. Hart refers, for example, to the mind which "refuses to treat the desire as part of itself," and to "the personality which has cut off and declined to acknowledge a portion of his mind[2]."

Even more necessarily anti-atomistic is the conception, basal to these forms of psychology, of the Unconscious itself. For however ambiguously defined, whether described in mainly physiological or in mainly psychological terms, whether regarded as primarily instinctive and emotional, or as creative entelechy—the Unconscious is always conceived as persistently exerting influence upon our conscious experience and thus, at least by implication, as a continuous and 'potentially' conscious being. In his recent book, *The Crisis in Psychology*, Driesch—whose soul-doctrine allies his system with the psychology of the Unconscious—confirms the conclusion that these systems are, in the wide sense of the term, personalistic. "In modern biology and in modern psychology," he says, "the concept of the *whole* plays a *fundamental* part....In the place of the 'sum-concepts,' *association* and *mechanics*, we now have the 'totality-concepts,' *soul and* entelechy[3]."

The preceding pages have, it is hoped, made clear a real and significant agreement between four otherwise divergent types of contemporary psychology. The frank acknowledgment of this agreement seems to the writer of prime importance. No science can permanently prosper in which, as in present-day psychology, practically every man thinks and writes as seems good in his own eyes without regard to any save a few close *confrères*. Certainly all who believe that the future in theoretical, as in applied, psychology lies with the anti-atomistic systems must ardently desire a greater *rapprochement* among their upholders. The least that would be gained by such affiliation would be the clearer thinking that must follow if advocates of these different systems, even without the attempt to obliterate the lines which divide them, would yet abandon their present policy of stressing only differences, never agreement. But it surely is not futile to hope for a closer convergence; and this paper has attempted to suggest the paths which lead to it. Extreme behaviourists, still repudiating introspectionism of the atomistic type and still retaining their conception of the reacting organism must admit consciousness as one observed form of this reaction. The *Gestalt*-psychologists must develop their neglected doctrine of the basal *Gestalt*, that is to say, the experiencing individual. The psychophysical personalists must concern themselves as much with the psychical as with the physical aspect of the conscious organism. Finally, the 'narrowly psychological' personalists, admitting as they do the methodological possibility of a psychophysical person, must recognize in this conception, rather than in their own doctrine of a merely conscious self, the centre of present-day anti-atomistic psychology.

---

[1] Cf. B. Hart, *The Psychology of Insanity*, pp. 50, 62 ff. Cf. also Stern, *op. cit.* p. 239 note: "Freud fasst die geistige Persönlichkeit wesentlich mechanistisch auf als Aggregat zahlloser psychischer Elemente."

[2] *Op. cit.* p. 125. Cf. the statement, p. 101, "We repress a complex."

[3] *The Crisis in Psychology*, p. 267. Cf. pp. 191 ff.

(*Manuscript received* 28 *October*, 1925.)

SOME EXPERIMENTS UPON INHIBITION AND SUGGESTION

By L. S. PENROSE.

(*From the Cambridge Psychological Laboratory.*)

I. *General description of the experiments* (pp. 180–182).
II. *Experiment* 1 (pp. 182–184).
III. *Experiment* 2 (pp. 184–186).
IV. *Experiment* 3 (pp. 186, 187).
V. *Experiment* 4 (pp. 187–189).
VI. *Experiment* 5 (pp. 189, 190).
VII. *Theoretical remarks* (pp. 190, 191).
VIII. *Summary of results* (pp. 191, 192).

I. GENERAL DESCRIPTION OF THE EXPERIMENTS.

THE following experiments were designed to investigate in an elementary way the effect of inhibition upon associated complexes. They are the converse of the more usual type of memory experiments, for they examine the tendency to remember incorrectly under given conditions.

If the revival of one psychological element such as a memory image or a reaction is accompanied by the revival of another element of this kind then these two elements are sometimes said to be 'associated.' It was my original purpose in making these experiments to examine the effect, if any, of the inhibition of one of two associated elements upon the ability to reproduce the other. The first scheme which I adopted for experiments in this direction was as follows. Two characters or forms (A and B) were presented together to the subject who was afterwards required to reproduce what he had seen, leaving out one of the characters (A). On carrying out an experiment modelled upon this scheme (Experiment 1) I observed that the inhibition of one factor (A) caused some hesitation in the reproduction of the other factor (B). But the effect was not very marked, and I decided to modify the scheme of the experiments by adding to it the suggestion of a particular mistake to be made in the reproduction of the element (B). This mistake was to be of the character of something erroneously added, which was of such a nature that it could be regarded in some measure as a substitute or compensation for the part left out (A).

The pattern of these new experiments can be set forth in the following manner, and it applies to Experiments 2, 3, 4 and 5.

(i) Allow two characters to be associated in the mind of the subject (call the characters A and B).

(ii) Suggest an alternative form (B') of one of the characters (B) which appears when the other character (A) is not present.

(iii) Forbid the reproduction of the constant element (A) and ask the subject for the other character (B).

(iv) Observe if there is any tendency to reproduce the alternative form (B') instead of the correct form (B) under these circumstances.

In the actual experiments, the alternative form (B') was constructed by adding to the original form an extra part similar to those which were already composing it. The advantage of this method was that it enabled the results of the experiments to be expressed numerically. For if the character (B) consists of several parts ($bbbb...b_n$), and the alternative consists in an increased number of these parts ($bbb...b_{n+m}$) the effect of inhibiting the character (A) which is associated with n parts can be measured by finding the average number of parts reproduced under varying circumstances and by noticing how near this is to the suggested number of parts, $n + m$.

It will thus be seen that the general nature of the Experiments 2, 3, 4 and 5 is the induction of a particular kind of mistake by an inhibitory command combined with a suggested compensatory reaction.

The experimental work was carried out partly in the Psychological Institute of the Vienna University and partly at the Cambridge Psychological Laboratory. The collective experiments were not performed by me personally, but were carried out according to my directions. These were done in Vienna, as were also the other experiments, as far as the first twenty subjects were concerned. The continuing of the work in Cambridge confirmed the previous conclusions, so that I felt justified in putting the two sets of results together. The subjects, numbering 98, were all adults.

Diagrams or groups of letters (which gave the data for the associated complexes) were shown to the subjects by means of the Ranschburg tachistoscope. Each figure was exposed by this means for 2 seconds in all cases except in the collective experiments, where the exposure was $1\frac{1}{2}$ seconds, the apparatus being a special one for the purpose and manipulated by hand.

Before showing the diagrams it was explained to the subjects that a series of groups of letters, pictures of houses, caterpillars, or whatever they were in the particular instance, was to be exhibited. The subjects were not asked to try to remember what they saw, but simply to give their full attention to each figure. As soon as the series of pictures had come to an end, the subjects were asked to reproduce a certain group or selection of the elements which had been shown, by drawing or writing it down on paper, under a given inhibitory condition, namely the leaving out of an element which formed part of this group. A limiting time of 20 seconds was allowed for this reproduction. I then questioned the subject about his drawing to make sure that the experiment had been understood properly; a few results were disqualified owing to failure to grasp what essential parts of the diagrams represented.

Control experiments were carried out by taking different subjects and requiring them to reproduce the same group of elements under the same conditions as before except that there was no inhibitory command. A set of supplementary experiments with corresponding controls were also performed (in connection with Experiments 2, 3 and 4) with the object of testing the effect of the inhibitory command when no suggestion of a compensatory mistake had been given. In these supplementary experiments only one diagram was shown to each subject instead of a series.

In order to neutralize as far as possible the effect of personal differences in the subjects in regard to accepting or rebelling against the inhibitory condition it was arranged for each subject to do some experiments and some controls. Thus, one subject had to do Experiment 1, Control 2, Experiment 3 and Control 4; whereas the next subject was given Control 1 and Experiment 2 to do, and so on, alternating each time.

II. EXPERIMENT 1.

To study the effect of inhibiting writing down a given letter upon the writing of associated letters.

The following set of pairs of letters was exhibited, the exposure for each pair being 2 seconds. Three different orders were used.

(i) FR KA QF KR NQ GK AQ AF QN RG QN GQ AR QK
(ii) QK AR GQ QN RG QN AF AQ GK NQ KR QF KA FR
(iii) QN RK QF AF KA GK AR NQ RG QK GQ QN FR AQ

The order (i) was shown to 15 subjects, (ii) which is the reverse of (i) was shown to 5, and 10 subjects were given (iii)—making in all 30 subjects. They were required to write down all the single letters they could remember having seen in these groups, without writing down the letter Q. A control was taken with 30 other subjects, divided up in the same way, who were required simply to write down all the single letters they could remember having seen.

The results were as follows:

(*a*) Experiment. (Q forbidden.)

The letter A was written by 30 subjects.
,, R ,, 25 ,,
,, F ,, 22 ,,
,, N ,, 20 ,,
,, G ,, 17 ,,
,, K ,, 15 ,,
Total number of correct letters written, 129.

(*b*) Control. (No inhibition.)

The letter Q was written by 30 subjects.
,, A ,, 26 ,,
,, R ,, 29 ,,
,, F ,, 16 ,,
,, N ,, 22 ,,
,, G ,, 16 ,,
,, K ,, 24 ,,
Total number of correct letters written, 133 (omitting Q).

Furthermore in both sets of experiments many mistakes were made. In the experiment (*a*) W was written five times, B twice, L, I, C, O and D once each; altogether twelve mistakes. In the control (*b*) sixteen mistakes of this kind were made and the selection was quite different. The letter P was written five times, T four times, H twice, M, S, W, K and C once each. In both cases only two subjects gave perfectly correct answers, but the number of subjects who had only one letter wrong or left out was eight in the experiment (*a*) and fifteen in the control (*b*).

These results appear to show that some effect was made on the reproduction of the other letters by the inhibition of Q. In the first place, the difficulty of writing down the correct set of letters was increased when Q had to be left out, and secondly the letter N which was most closely associated with Q in the series (never appearing without Q) was written down fewer times when Q was forbidden.

The experiments which now follow were devised, on the strength of the first batch of results of Experiment 1, in order to produce if possible a more definite effect by an inhibitory command. Diagrams were substituted for letters in the hope of avoiding to some extent extraneous associations. And the previous suggestion of a mistake was combined in what was shown to the subject; so that the effect of an inhibitory command in tending to produce a suggested mistake could be studied.

III. EXPERIMENT 2.

To examine the effect of preventing the drawing of part of a simple figure with special relation to the making of a particular mistake which has been previously suggested.

A series of pictures representing diagrammatic caterpillars, which is shown in Fig. 1, was presented to 40 subjects by means of the Ranschburg apparatus, each diagram having 2 seconds' exposure. Half these subjects were then required to draw the last picture from memory, leaving out the head of the caterpillar, and the remainder were simply asked to draw the last picture. Now all the insects except the last one have four segments in the body, whereas the last one has only three. In addition to this, the last figure but one represents an insect without a head and with four segments. Hence, unless the subject attends well to the essential peculiarity of the last diagram he may be expected to reproduce what he has already seen in the previous one when asked to draw the last picture and to leave out the head.

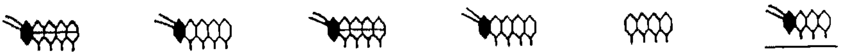


Fig. 1. The series used in Experiment 2.

(The last diagram, which was the one required to be drawn, is underlined.)

The actual result of the investigation was as follows:

(*a*) Experiment. (Head not to be drawn.)

Out of 20 subjects: 12 drew 3 segments = 36
7 „ 4 „ = 28
1 „ 5 „ = 5
Total = 69 segments.
Average for each subject = 3·4 segments.

Seeing that the correct number was 3, and that 4 were suggested, this result indicates that the suggestion of one extra element together with the inhibition of another is sufficient to cause an increase in the number of elements drawn. The conclusion can be tested by reference to the control result.

(*b*) Control. (No inhibition.)

Out of 20 subjects, apart from drawing the head clearly,

2 drew 1 segment only = 2
6 „ 2 segments = 12
8 „ 3 „ = 24
4 „ 4 „ = 16[1]
Total = 54 segments.
Average for each subject = 2·7[2]

This shows that the effect of the suggestion of the four elements without the inhibition of an element does not have the same effect as before in the experiment. And the difference between the two results gives an average value to the effect of the inhibition, namely, ·75 of an element, or if we prefer, 25 per cent. increase on the correct number of elements. It is interesting to note that the number of legs of the insects drawn was correspondingly larger in the experiment than in the control, totalling 45 as against 37 in the latter, the correct number being in each case 60.

In order to test the validity of my conclusion further, I carried out an extra set of experiments, showing the last diagram only for 2 seconds, so as to rule out the effect of the suggestion of a mistake. These supplementary experiments were carried out upon 10 subjects and 10 other subjects were given the corresponding control to do, and the following figures were obtained:

(*a*) Experiment. (Subjects forbidden to draw the head—no suggestion.)

Out of 10 subjects:

10 drew 3 segments = 30
Average for each subject = 3·0 (the correct number).

(*b*) Control. (Subjects simply required to draw the picture they had seen—no inhibition and no suggestion.)

Out of 10 subjects, each of whom drew the head as well,

9 drew 3 segments = 27
1 „ 2 „ = 2
Total = 29
Average for each subject = 2·9

---

[1] In two of these cases the head was counted as a segment owing to uncertainty on the part of the subject—otherwise the result would be even more favourable

[2] The correct number is 3.

The slight difference between these results may be attributable to the inhibition seeing that it lies in the same direction as in the previous case. The number of legs drawn was also increased as before in the experiment with inhibition, standing at 32 as against 20 in the control, the correct number being in this case 30.

IV. Experiment 3.

(*The purpose of the investigation is the same as in Experiment* 2.)

In this experiment a new series of diagrams was used in exactly the same way as in Experiment 2. The diagrams depicted beetles, each having six legs except the last one which had only four. The beetle shown next to the last one (as well as two others in the series) had six legs but no antennae and it was this figure which I expected would be frequently drawn when the subjects of the experiment were required to draw the last beetle without its antennae. The results of this experiment with control, carried out on 40 subjects, are given here.

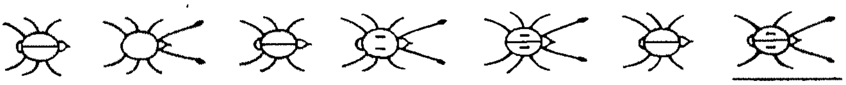


Fig. 2. The series used in Experiment 3.
(The last diagram, which was the one required to be drawn, is underlined.)

(*a*) Experiment. (Subjects required to draw the last beetle but not to draw the antennae.)

Out of 20 subjects: 1 drew 7 legs = 7
10 „ 6 „ = 60
9 „ 4 „ = 36
Total = 103
Average per subject = 5·1

(*b*) Control. (Subjects required to draw the last beetle—no inhibition.)

Out of 20 subjects: 5 drew 6 legs = 30
15 „ 4 „ = 60
Total = 90[1]
Average per subject = 4·5

---

[1] Five subjects omitted to draw antennae or were uncertain as to whether they had drawn antennae or legs; in these cases all appendages were counted as legs, else the result would have shown a greater difference between the experiment and the control.

As the correct number of legs is four and six is previously suggested, it can be seen that there is an increase in the mistake made, in the direction suggested, when inhibition is present. The increase averages out to ·6 element for each subject, an increase of 15 per cent. upon the correct number of elements.

A supplementary investigation was made here as in Experiment 2 to test the effect of this particular inhibition, without previous suggestion of a mistake, by showing the last diagram only and proceeding otherwise just as usual.

(*a*) Experiment. (Antennae forbidden—no suggestion.)

Out of 10 subjects: 7 drew 4 legs = 28
3 „ 6 „ = 18
Total = 46
Average per subject = 4·6

(*b*) Control. (No inhibition or suggestion.)

Out of 10 subjects: 9 drew 4 legs = 36
1 „ 6 „ = 6
Total = 42
Average per subject = 4·2

The results of this experiment seem to bear out what is generally known, that in copying any figure from memory there is a tendency to increase the number of particular elements, but they also seem to show that this increase is greater when suggestion of more elements or inhibition of part of the figure is an extra condition.

V. EXPERIMENT 4.

(*Object the same as Experiment* 2.)

A series of pictures representing flowers were shown in this experiment under the same conditions as in the last two experiments. The last flower had only four petals, whereas all the others had five, but three of these had no centre drawn. In the experiment the subjects were required

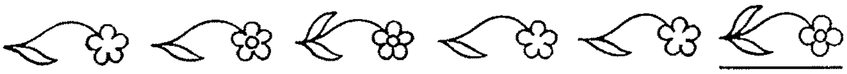


Fig. 3. The series used in Experiment 4.

(The last diagram, which was the one required to be drawn, is underlined.)

to draw the last flower leaving out the centre. I imagined that this inhibition might induce the mistake to be made of drawing five petals instead of four. The following figures were obtained:

(*a*) Experiment. (Subjects forbidden to draw the centre of the flower.)

Out of 20 subjects: 10 drew 5 petals = 50.
10 „ 4 „ = 40
Total = 90
Average per person = 4·5 petals.

The tendency, observable here, to draw five petals instead of four is partially attributable to the leaving out of the centre, for the control experiment gave the following result:

(*b*) Control. (No inhibition.)

Out of 20 subjects: 6 drew 5 petals = 30
14 „ 4 „ = 56
Total = 86
Average per person = 4·3 petals.

The average increase here of ·2 petal appears to be connected with the inhibitory command to leave out the centre. But with regard to the incidental feature of drawing the leaves, 32 were drawn in the experiment against 35 in the control, showing that the result obtained with incidental features in Experiment 2 (legs) does not hold unless the suggestion applies to them.

A collective experiment with its corresponding control was carried out with this same series of pictures and the results are given below.

(*a*) Experiment—collective. (Centre of flower left out.)

Out of 11 subjects: 4 drew 4 petals = 16
6 „ 5 „ = 30
1 „ 6 „ = 6
Total = 52
Average for each person = 4·7 petals.

(*b*) Control—collective. (No inhibition.)

Out of 10 subjects: 5 drew 4 petals = 20
5 „ 5 „ = 25
Total = 45
Average for each person = 4·5 petals.

A slight increase in the average number of elements drawn (·2 petal) is observable here in connection with the inhibition. In the experiment 16 leaves were drawn altogether whereas in the control the number was 14, showing on the average very little difference between the two cases.

A set of supplementary experiments, in which only the last diagram of the series was shown, was also carried out in order to test the effect of the inhibition without previous suggestion. In this case no change could be detected as the result of the inhibitory command.

(*a*) Experiment. (Centre to be left out, but no suggestion.)

Out of 10 subjects: 2 drew 5 petals = 10
8 „ 4 „ = 32
Total = 42
Average per subject = 4·2

(*b*) Control. (No inhibition and no suggestion.)

Out of 10 subjects: 2 drew 5 petals = 10
8 „ 4 „ = 32
Total = 42
Average per subject = 4·2

The tendency to draw too many elements in any case can, however, be inferred from this result, and the rest of the experiment shows that the tendency is reinforced by inhibition combined with suggestion of too many elements.

VI. EXPERIMENT 5.

(*Object the same as Experiment* 2.)

This experiment was carried out collectively only, upon two groups of subjects numbering 12 and 11, for the experiment and control respectively. The series of diagrams used represented houses (Fig. 4). In three cases the houses had four windows drawn; the last diagram but one, and the one before it, showed four windows and no door. The subjects of the experiment were required to draw the last house, which had only three windows, and they were told not to draw the door. In the control the

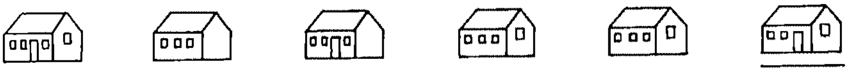


Fig. 4. The series used in Experiment 5.

(The last diagram, which was the one required to be drawn, is underlined.)

subjects were only required to draw the last house. The results were as follows:

(*a*) Experiment—collective. (Door not to be drawn.)

Out of 12 subjects: 1 drew 2 windows = 2
7 ,, 3 ,, = 21
4 ,, 4 ,, = 16
Total = 39

Average per person = 3·25 windows.

(*b*) Control—collective. (No inhibition.)

Out of 11 subjects: 4 drew 2 windows = 8
3 ,, 3 ,, = 9
3 ,, 4 ,, = 12
1 ,, 5 ,, = 5
Total = 34

Average per person = 3·09 windows.

The result shows an increase in the number of elements drawn, when a more or less equivalent element is inhibited; in a similar way to that observed in the last experiment.

VII. THEORETICAL REMARKS.

In devising the above experiments, I had it in view to obtain results in the form of definite reactions on the part of the subjects, such as drawing or writing, and I was concerned with the effect of inhibitory commands upon such reactions. I was not primarily concerned with the reasons which were stated by the subjects to be the cause of their reactions such as could be found by introspection. Nevertheless reasons of this kind were often volunteered by subjects after they had finished writing, especially in Experiment 1. For example, subject 23 alleged that he was unable to write down K because it reminded him of 'Kaiser' and 'Kassa,' two unpleasant ideas to him at that time. Subject 3 attributed not having written down N in the same experiment to the association of this letter with Napoleon, whom he detested. Conversely, subject 4 complained of having been 'put off' by not being allowed to write down Q, since Q suggested pleasant things to her, like Queen.

A psychologist of the Freudian school on viewing the sets of diagrams used for the last four experiments and considering the character of the inhibitions imposed might see a relation between the idea of the removal of a part from a diagram and the idea which is characteristic of what is called the 'castration complex.' The selection of data for the experiments is however in this respect intentional. The compensatory mistake resulting from removing a part from an associated complex is comparable to one of the substitution mechanisms insisted upon by Freud, which occurs specially in connection with the 'castration complex.' Furthermore, the compensation produced in the experiments is in the nature of a regression to an idea previously formed; the subject tends to go back to the memory of a previously shown figure when faced by a difficulty in reproducing the last one. Therefore, one may see perhaps in the results of the Experiments 2, 3, 4 and 5, the effect of the Freudian mechanism of frustration being followed by regression and compensation in a miniature form (for a mistake is, according to that doctrine, a miniature neurosis).

This way of looking at the question may explain what seems an important and rather puzzling point in the net result of experiments, namely, that the effect of the inhibition is, in general, not to increase the number of people who make mistakes but to alter the value of the mistakes made. Actually the total proportion of correct results appears to be unaffected by the inhibition. Taking all the experiments together, 49 out of 144 subjects gave correct answers, whereas, in all the controls 48 out of 142 subjects gave correct answers. It appears probable that mistakes cannot be induced by these methods but that a mistake which is about to be made in any case can be to some extent anticipated.

Against this one may note that the effect of suggestion on the number of the correct answers is considerable. When no suggestion was given 32 out of 60 subjects were correct, but when suggestion was given only 48 out of 120 subjects gave correct answers in the corresponding experiments. This effect is, however, partly to be accounted for by the fact that in the cases where suggestion was given a series of diagrams instead of only one had to be comprehended.

VIII. SUMMARY OF RESULTS.

In order to get a rough idea of the relative effects of inhibition and suggestion in these experiments one may take the results of the last four experiments and add them together, for in all cases the results agreed.

(i) Experiments with inhibition and suggestion.

Correct number of elements = 300
Number of elements drawn = 353
Number suggested = 403

Increase on correct number, 17·8 per cent.

(ii) Controls with suggestion and no inhibition.

Correct number of elements = 293
Number of elements drawn = 309
Number suggested = 394

Increase on correct number, 6·7 per cent.

(iii) Experiments with inhibition and no suggestion.

Correct number of elements = 110
Number of elements drawn = 118

Increase on correct number, 7·1 per cent.

(iv) Controls with no inhibition and no suggestion.

Correct number of elements = 110
Number of elements drawn = 113

Increase on correct number, 2·8 per cent.

The following general conclusion can, I think, be formed on the results of these experiments:

There is a tendency to increase the number of similar elements in a figure when it is reproduced from memory, yet this tendency is augmented either when a group containing more of these elements has been previously seen, or else when the reproduction of some part of the group in close relation to these elements is inhibited.

The combination of suggestion and inhibition of this kind produces a greater effect than the sum of the effects of the two conditions given separately.

(*Manuscript received* 26 *August*, 1925.)

ON THERMAL SENSITIVITY AND THE NATURE OF SENSORY ADAPTATION

By ELMER CULLER.
(*From the University of Illinois, U.S.A.*)

1. *Experimental procedure* (pp. 193, 194).
2. *Experimental results* (pp. 194–197).
3. *A theory of sensory experience* (pp. 197, 198).

In the course of experiments upon thermal sensations[1] I came upon certain facts whose theoretical implications about the nature of adaptation and how it relates to sensory function may be of sufficient novelty and consequence to warrant a brief exposition at this place. I first shall indicate the procedure and principal findings of the former study and then propose the inferences I believe these facts to justify.

1. *Experimental procedure.*

Take several jars of water differing but little (·05° to ·30° C.) in temperature. An observer, who is kept in ignorance of their actual relation, compares them repeatedly by successively immerging the four fingers of both hands in a pair of jars and then reporting in each case which of the two is warmer. By Urban's $\Phi(\gamma)$ process we then calculate the limens, absolute and differential. There being no real zero-point in thermal stimulation, no absence of temperature as there may be of light or sound, we are reduced to choosing a level of reference arbitrarily (this level being the adaptation-temperature) and then maintaining it. This adaptive level in one series remained at 32° throughout (approximate skin-temperature of the fingers), while in the other it ranged from 16° to 44°, being always identical with the standard stimulus. Now if we find, when adapted to 32°, that 36·15° is liminally warmer than 36·00°, we say that + ·15° is the difference-limen for Warm at 36°; whereas if we find, when adapted to 36° (this temperature then being relatively neutral), that an increment of ·05 suffices to evoke a perceptible warmth by our criterion, then + ·05 is the absolute limen for Warm at 36°. This

---

[1] Culler, Elmer, "Thermal discrimination and Weber's law," *Archives of Psychology*, 1925.

A.L. in a word measures the degrees of change in thermal stimulation which barely serves to arouse under stated conditions a positive thermal response (warm or cool) in a mechanism previously adapted; while the D.L. measures how widely two thermal stimuli must differ if they are to evoke a differential (comparative) response (warmer or cooler).

2. *Experimental results.*

So much for procedure; the major findings are as follows:

(*a*) We find that an ordinary observer under these conditions can reliably appreciate a thermal change of ·03° to ·05° C. (A.L.). Discrimination is most efficient in the region of 28°, which is about 4° below surface-temperature in the fingers. The reason for this may well be that the skin, when immersed in water of 28°, loses heat at about the same rate as when surrounded by air at 20° (68° F.); the latter is usually said to be the 'normal' air-temperature, for man, most conducive to comfort and efficiency. This liminal value, which marks the thermal sensitivity of the organism, increases slowly on each side of 28°; but even at 16° and 44°, where numbness or pain begins to appear, the D.L. does not exceed ·08°. Several experienced psychologists and other scientists have professed amazement at the 'astounding' thermal sensitivity of the human organism—a capacity which is indeed sufficiently surprising and which perhaps few people suspect. Withal it is not strange that the organism is well prepared to accommodate to thermal changes; this mode of reaction begins far down in the animal kingdom (protozoa) and is so intimate, so nearly related with internal economy and general welfare of the body, that it needs to function with no little efficacy. There may however be a wide disparity between homologous areas of the same organism; the left-hand, *e.g.* may be twice as responsive as the right, even though both are to all appearance normal.

(*b*) We find that the relation known as Weber's law (the discriminable difference being a fixed proportion of the absolute degree of stimulation) holds not at all for *differential* sensitivity, to which it has always been traditionally applied, but only for *absolute* limina. With the latter we have a clear linear relation between limen and degree of stimulation, that is, a constant Weber ratio; whereas in the former we have a decreasing relation, the ratio becoming progressively smaller (negative acceleration) as the temperature departs from the adaptive level, 32°. This fact, whose reality is amply confirmed with data from all our observers and by various methods, seems to be of first-rate consequence not only for Weber's law but for thermal theory as well. While it has been demonstrated only for temperature[1], the question is at least worthy of attention, whether the same be not true for other forms of sense-perception (vision, audition) as well.

(*c*) The Weber ratio for the A.L. is the lowest ever reported for any sense. These constants are usually given, in percentage, as about 25·0 for sound, 4·0 for pressure, 1·0 for vision; with temperature it reduces to 0·4. This low ratio is of more than merely technical importance; it proves the organism to be about equally sensitive, that is, about equally adaptable to temperature within what may roughly be called the normal range of stimulation (16° to 44°)[2]. We have hitherto looked upon vision as the sense most uniformly responsive throughout the ordinary range of excitation, its liminal increase being only 1 per cent. of the stimulus-rate; the thermal senses now appear to be yet more uniformly adapted to normal stimulation, the threshold rising with but ·4 per cent. of the stimulus-rate. This shows again how well and how regularly the organism is prepared to cope with thermal changes.

Other evidence as to the time required for adaptation[3] and the degree of adaptability at various levels[4] combine with the facts above stated in the following theory of thermal sense-function as related to adaptation:

1. The absolute limen is an inverse measure of an organism's adaptive capacity on a given temperature level.

2. The differential limen is a direct measure of the rate with which adaptation (to a given temperature) is proceeding.

3. It follows that the more rapidly and completely adaptable a receptive mechanism is, the *smaller* its absolute limen and the *larger* its differential limen. This inverse relation of the two measures of discrimination (A.L. and D.L.) seems to be fundamental and has been wholly neglected hitherto.

---

[1] A recent study reveals precisely the same difference in the case of pressure. See H. Schriever, "Über die gültigkeit des Weberschen gesetzes im gebiete des drucksinns," *Archiv. f. d. ges. Psych.* LI, p. 137, 1925.

[2] At 44° (in water) pain is already perceptible and increases rapidly thereabove; while at 16° likewise marked traces of numbness and other symptoms of imperfect function begin to appear.

[3] Holm, K. G., "Dauer der temperaturempfindungen," *Skand. Archiv. f. Phys.* XIV, p. 242, 1903.

[4] Voigt, A., "Über beurteilung von temperaturen unter d. einfluss d. adaptation," *Zt. f. psy.* LVI, p. 344, 1910.

4. What we call the 'intensity' of a given warmth or cold is therefore an immediate symptom or measure of the rate with which the organism is accommodating to the stimulus.

We may clarify these propositions, in case they sound paradoxical or obscure, by reference to a simple mechanical analogy—the ordinary balance. The sensitivity of a balance (E) is defined by the formula

$$E = \frac{da}{dD},$$

where a is the angle of the beam with the horizontal and D is the difference in weight suspended from the two arms; the farther the beam displaces in angular width after a stated increment (D) in one pan, the more 'sensitive' is the balance. In other words, a delicate mechanism 'adapts' readily and fully to a minimal disturbance of energy-distribution ('stimulation') and assumes, in course of the process, a wholly new position of equilibrium or repose; whereas a cruder mechanism will give no perceptible response at all to a change so minute. The degree of displacement (da) induced by the energy-increment (dD) measures what we may call the absolute sensitivity of the balance, dD being the 'absolute limen' and da the index of adaptability (degree of displacement in the beam, the level of reference); this 'limen' rises and 'adaptability' falls as we depart from the horizontal on either side, just as in thermal discrimination on each side of 28°. So much for the relation of 'absolute limen' to 'adaptability.' The D.L., however, we conceive as deriving from the *rate* of adaptation, which corresponds to the angular *velocity* with which the beam departs from horizontal and moves into a new position of repose. Clearly the beam will fall faster with a large than with a small D (excess in one pan), and a sensitive beam faster than a less sensitive one. If we have a chemical balance responsive to a D of ·01 mgm. (low 'absolute limen') and add 10 gm. to one pan (making $D = +10$ gm.), the beam will of course descend with great velocity, in fact just about as rapidly as if we used 20 or 100 gm. Now suppose an observer were given the task of judging, from the beam's *rate of fall* over a given unit of time or at a particular point of its course, which was the heavier weight (10 or 20 gm.), it would clearly be much harder to distinguish these high and almost identical angular velocities than the lower and more unlike velocities in a cruder balance. When the balance is made more and more sensitive, it responds with ever-increasing celerity and ease until it reaches the theoretical limit where *every* D evokes the same maximal adjustment; then no D of course can be distinguished from any other. That a delicate mechanism of high sensitivity (low 'absolute limen') should have a larger 'differential limen' than does a crude mechanism of low sensitivity (high 'absolute limen'), instead of being anomalous or paradoxical, turns out therefore to be logical and necessary.

3. *A theory of sensory experience.*

In the light of this conception the purpose of sensory experience works out somewhat as follows. When exposed to stimulation the organism at once begins a process of adjustment leading to eventual 'adaptation.' This change is attended by what is commonly called a 'quantitative' alteration in the experience (warm) which began upon incipience of stimulation and now persists in diminishing degree so long as the adjustive process continues; that is, the sense-impression grows fainter and fainter until it may be almost imperceptible. *Any form of sense-experience* (warm, cool, pain, colour) is, then, a mark or sign that we are, for the time, *ill-adapted* or *mal-adjusted* to our situation, like the balance before it attains a new position of repose; only when the sense-impression stops have we achieved a measure of equilibrium which relieves us of further efforts to accommodate. A situation to which we are wholly adapted will then be without sensory effect, without warm or cold, sound or pain. The sensory aspect of experience is but one phase of the adjustive processes by which the organism gets on with its environment; but the fundamental feature is *not* the sense-experience *but* the adaptive process by which the former is got rid of. The same *gestalt*, to be sure, which is responsible for the sense-impression eventuates also in one form or other of adaptive behaviour; this activity commonly changes the situation so that a new process of sensory adaptation is needed and inaugurated; but the end-term of the process ever remains a state of equilibrium whose sensory aspect is the condition which we call 'adaptation.' The two phases of adaptation indeed (sensory and motor) are complementary modes of organic adjustment. An organism on the one hand meets the needs of existence by movements of escape or control, while at the same time it achieves direct intrasomatic accommodation (fading of the sense-impression) so far as organic resources permit.

Slow and imperfect adaptability then, as in pain, proves that the organism is being affected with modes of stimulation which it is not prepared (adapted) to meet; while easy and high adaptability proves that it is well equipped to cope with the forms of energy now impinging upon it. What we call 'experience' is largely composed of these adjustive processes, initiated by the multifarious stimuli of the world about, one process relieving another and few coming to completion. Whatever else be true of a limen, therefore, it reveals how much a given situation need be changed to inaugurate a new effort of accommodation by the responsive mechanism. Just as the balance is ever 'seeking' a position of repose, so the organism is always in course of restoring equilibrium after the attacks of various stimuli.

(*Manuscript received* 26 *August*, 1925.)

FACTORS IN MENTAL AND SCHOLASTIC ABILITY[1]

BY H. G. STEAD.

I. *Introduction* (pp. 199, 200).
II. *The Tests* (pp. 200–203).
III. *Distributions of Scores* (pp. 203–205).
IV. *Factors in successful performance* (pp. 205–207).
V. *Is a 'general factor' present?* (pp. 207–212).
VI. *Conclusions as to a 'general factor'* (pp. 212–216).
VII. *Three significant factors in mental ability* (pp. 216–219).
VIII. *Some individual cases* (pp. 219, 220).
IX. *Summary* (p. 221).

I. *Introduction.*

THE following experimental investigation was made in the hope that it might throw some light upon the factors operative in mental and scholastic ability. Four groups of tests were applied to five groups of subjects and the resulting correlations were treated statistically to see to what extent they could be claimed to exhibit the operation of a general factor, (1) within each group of tests and (2) in the tests as a whole.

The Subjects.

In all 227 subjects were tested. These were classified into the following groups:

(1) *Group A.* Boys. Ages 11–12 (where ages are mentioned they are chronological ones, unless otherwise stated, and mean that all subjects in the group had reached the first age, but were not yet the second). 39 cases. These were all boys from an Elementary School.

(2) *Group B.* Boys. Ages 12–13. 135 cases. This group consisted of 127 boys actually in an Elementary School and 8 boys who had just (at the time of testing) been admitted from the Elementary School to a Secondary one.

---

[1] Being an abstract of a thesis approved for the degree of Ph.D. in the University of London.

(3) *Group C.* Boys. Ages 13–14. 61 cases. All were Elementary School boys.

(4) *Group D.* Boys. Ages 14–15. 17 cases. All were Secondary School boys.

(5) *Group E.* Girls. Ages 13–14. 25 cases. All were Elementary School girls.

The first three of these groups were of sufficient size to be expected to give reliable results. The other groups were smaller, but the results show some points of interest, and for that reason are included in this report.

II. *The Tests.*

All the subjects were given three sets of tests and were rated for a series of character traits by a teacher who had known them for some considerable time. The various tests are outlined below.

(A) *Mental Tests.*

A series of eight tests was given. Each test was given separately and the instructions and examples were given in exactly the same form and words to each set of subjects tested. The tests were: (1) Opposites, (2) Analogies, (3) Synonyms, (4) Instructions, (5) Completion, (6) Absurdities, (7) Number Series and (8) Cipher. These tests are all too well known to need further description here[1]. In (1), (2) and (3) 25 examples were given; in (4), 20; in (5) a short story with 32 words omitted; in (6), 20 absurdities; in (7), 20 series, and in (8), 5 single words followed by 4 short sentences. A time limit was fixed in all the tests except (4), (5) and (6). All these tests were given as group tests.

(B) *Motor Tests.*

Six tests of motor ability were given. These were:

(1) *Tracing.* The subject had to trace a line between the two lines given in the figure shown opposite. The score was the number of times the bounding lines were touched. Two tests (upon different figures) were given to each subject. Before commencing this test each subject was practised between two straight lines in order to obtain approximately the same speed in each case.

(2) and (3). *Match stick insertion.* Right and left hands. The subject was given an open match box which he held in his left hand. In front of him was a board in which had been bored a number of holes, irregularly spaced, of such a size that a match could just be inserted in each.

---

[1] See Burt, *Mental and Scholastic Tests*, London, 1922, 223–38, and Ballard, *Group Tests of Intelligence*, London, 1922, 104–29.

The subject was requested to insert as many as he could in one minute, this number forming the score. The test was afterwards repeated with the hands reversed and the result counted as a separate test.

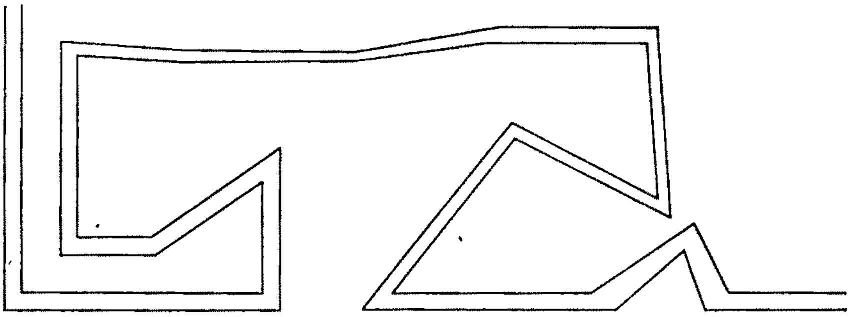


Fig. 1. Example of Figure used in Tracing Test.

(4) and (5). *Steadiness*. Right and left hands. A sheet of metal was placed in front of the subjects. In this sheet a hole, ·5 cm. in diameter, was made, and the subject was given a thin metal rod to be held in his right hand. He was then instructed to put the rod through the hole and hold it as steady as he could without touching the sides of the hole. The score was the average number of touches made in three trials of one minute each. The test was repeated with the left hand, and the result counted as a separate case. In this test the arm was not supported at the elbow and in most cases an electric device was attached by which the number of touches could be counted.

(6) *Matches in Box*. Thirty matches were laid out on a sheet of paper in an irregular fashion (the same for all subjects) and the subjects were instructed to pick them up with the right hand, one at a time, and place them with their heads all in one direction, in a match box held in the left hand. The score was the number of seconds taken to perform the task.

Of these tests (1) was given to small groups, and the remainder were given as individual tests.

(C) *Scholastic Tests*.

All the subjects were given the following scholastic tests:

(1) *Spelling*. The 'graded vocabulary' test of Dr Burt[1] was used, and the score was obtained by the formula given by him.

(2) *Arithmetic—Rules*. Burt's[1] mechanical arithmetic test was used.

---

[1] See Burt, *Mental and Scholastic Tests*, London, 1922, 339–65.

(3) *Arithmetic—Problems.* Burt's[1] problems were used.

(4) *Reading—Speed.* The discontinuous ungraded test of two- and three-letter monosyllables given by Dr Burt[1] was used.

(5) *Reading—Comprehension.* All the subjects were given the continuous prose test of Dr Burt[1]. After they had had five minutes in which to study this they were given a sheet based upon the interrogatory given by Dr Burt[1], but in the form of a 'completion' test. The score was the number of missing words correctly inserted.

(6) *Essay.* All the subjects wrote an essay upon 'School'; without having received any preliminary notice, or instructions. These essays were all marked by the present writer, in order to secure uniformity in marking as far as possible.

(7) *Drawing.* All the subjects were instructed to draw a man, woman, boy, or girl doing anything they liked. These again were all marked by the writer. Both here, and in the case of the essays, the whole of the exercises were first placed in groups of approximately equal value and then each group was dealt with, transferences from one group to any other being made if necessary.

The above scholastic tests were all given as group tests, except (4).

(D) *Character Ratings.*

All the subjects were rated for six character traits by a teacher who had been well acquainted with them for at least a year—in some cases the acquaintance extended to over four years. The following instructions were issued.

Method of Marking Character Traits.

Please mark each character trait in the attached list according to the following scheme.

Pupils showing

		Marks
(*a*)	Trait very strongly marked	+ 3
(*b*)	Trait considerably above the average	+ 2
(*c*)	Trait slightly above the average	+ 1
(*d*)	Trait of average strength	0
(*e*)	Trait slightly below average	− 1
(*f*)	Trait considerably below average	− 2
(*g*)	Trait markedly deficient	− 3

The majority of subjects will probably lie within the middle three groups, but this is not necessarily so. It is usually best to mark the best, and the worst, pupils first, then fix the position of the rest with reference to these.

---

[1] See Burt, *Mental and Scholastic Tests*, London, 1922, 339–65.

In addition each 'ranker' was supplied with the following notes and instructed to study them carefully before commencing to rank the subjects.

Notes on character traits.

(1) *Persistency*—to be measured as the tendency to continue tasks in the face of obstacles. The attitude of the pupil towards 'problem' work may assist in this rating.

(2) *Trustworthiness*—to be measured as the tendency of the pupil to carry out what he considers to be his duty.

(3) *Conscientiousness*—to be measured by the tendency of the pupil to put his best into any allotted task. Any pupils who try their best (irrespective of the results attained) should be graded high.

(4) *Changeability*—to be measured by the tendency to wish to change continually the task in hand. The trait to be measured here is not that which causes a pupil to give up one task and take up another in face of obstacles to the successful carrying out of the first, but just change for the sake of change.

(5) *Leadership*—to be measured by the degree to which the pupil exercises any influence over other pupils. This trait is to be measured by the *amount* of his influence, not by his *desire* to exert influence.

(6) *General excellence of character.*

A special column was provided in which could be entered factors which specially influenced any rating. It is worthy of note here that most raters agreed that the task of rating was difficult, but that they thought they had been fairly successful in accomplishing it[1].

III. *Distributions of Scores.*

There are a few points of interest in the distributions of the scores in the various tests. The girls (the group is too small to use as a basis for any definite deduction) seem to be decidedly above the boys of the same age in the mental tests, slightly above them in most scholastic ones, but decidedly below them in motor tests. This is in general agreement with Burt's[2] finding for these ages except with reference to the motor tests, which he did not use, although it is significant that he found the boys he tested superior to the girls in Handwork. Another point of interest is that the boys of 12–13 scored better than those of 13–14 (on the average) or 14–15, in all motor tests except those for steadiness. (The smallness of the latter group renders the conclusion for this group uncertain, but the 13–14 group contained 61 cases.)

But it is when the positions of individual cases in each type of test within each group are considered that some points of greater interest arise.

---

[1] It must be remembered, however, that the figures for the character traits cannot be claimed to have the same degree of reliability as those for the mental and scholastic tests.

[2] See Burt, *Mental and Scholastic Tests*, London, 1922, 193, 399–410.

Of the boys who occupy the first ten positions (*i.e.* the upper quarter) in the mental tests (total score) in Group A (39 boys, ages 11–12), six are in the upper quarter in the total score for the motor tests. In Group B, out of the first 34 boys (the upper quarter, again) in the mental tests, eight are in the top quarter in the motor tests. In Group C the corresponding figures are 1 out of 15. Expressed as percentages, this means that in Group A 60 per cent. of the upper quarter in the mental tests are in the upper quarter in the motor tests, while in Group B the corresponding percentage is 23·5, and in Group C, 6·6.

There seems to be some indication here of a possibility that ability in motor and mental activities hang together more closely in the case of younger children than they do in the cases of older ones. This is worthy of comparison with Muscio's[1] results. He found no correlation between mental and motor activities in *adults*. The above evidence would seem to bear this out, but it suggests that this may be due to the development of one set of interests at the expense of another. Some further evidence is given later on.

The following table gives the correlations between the total scores in the mental, motor and scholastic tests, and in the character ratings, for each group. A 'total' character rating was obtained by adding the scores allotted to each trait (algebraically).

TABLE I. *Correlations between Group of Tests in the various groups of subjects.*

Group	Scholastic and mental	Scholastic and character	Scholastic and motor	Mental and character	Mental and motor	Character and motor
A. 39 boys, 11–12	·538	·323	·333	·323	·528	·177
B. 135 boys, 12–13	·583	·374	·240	·271	·042	·167
C. 61 boys, 13–14	·538	·251	·032	·167	– ·094	·073
D. 17 boys, 14–15	·889	·178	·282	·052	– ·161	·385
E. 25 girls, 13–14	·635	·390	– ·122	·240	– ·032	·024

(P.E. varies from ·038 in Group B to ·162 in Group D.)

These figures show a general uniformity which is worthy of note. Further, when the table of correlation coefficients between the various groups of tests for each group of subjects is arranged so that the group of tests having the highest total correlation with all the other groups is at the top and so on, the order is remarkably consistent throughout the groups, as is seen by Table II.

Group D (17 boys, 14–15) is a 'selected' group, for all the subjects in it were pupils at a Secondary School. Omitting this group from the discussion on this account the following facts appear and are worthy of consideration:

---

[1] See this *Journal*, 1922, XIII, pt. 2, 184.

(1) The correlation between Scholastic and Mental tests is approximately the same throughout (average $r = \cdot 573$).

(2) The correlation between Scholastic tests and character traits is appreciably higher (in every group except A) than that between the mental tests and character traits.

(3) The correlation between motor tests and the tests belonging to each of the other groups shows a tendency to become smaller as the subjects become older.

TABLE II. *Order of total inter-group correlations in various groups of subjects.*

	Group A. 39 boys, 11–12	Group B. 135 boys, 12–13	Group C. 61 boys, 13–14	Group D. 17 boys, 14–15	Group E. 25 girls, 13–14
Order of total intercorrelation (highest on top)	Mental	Scholastic	Scholastic	Scholastic	Scholastic
	Scholastic	Mental	Mental	Mental	Mental
	Motor	Character	Character	Character	Character
	Character	Motor	Motor	Motor	Motor

These facts indicate that there is a good deal in common between mental and scholastic tests, and more in common between character and scholastic tests than between character and mental tests, since the correlation between the last-mentioned pair is less than that between those mentioned first. They also indicate that success in motor and mental tests 'hangs together' more closely in younger subjects than in older ones.

IV. *Factors in successful performance.*

Now the factors which contribute to the successful performance of any series of activities must obviously belong to one of the following classes:

(*a*) *General factors*—*i.e.* factors which enter into all the activities in question.

(*b*) *Group factors*—*i.e.* factors which enter into a number of the activities only.

(*c*) *Specific factors*—*i.e.* factors which enter into one activity only.

It will be noticed that (*a*) and (*c*) constitute the limiting cases of (*b*)—(*a*) when a group factor enters into *all* the activities and (*c*) when it enters into *one* only.

The correlations found above may, then, be due to either (or all) of the following causes. There may be:

(1) A common factor (or factors) which enters into all the activities upon which the scores of all the tests in each group are based.

(2) Group factors which may be either common to all the activities within any group or common to the activities within two or more groups.

(3) Factors which may be common to two (or more) of the tests within any group, *i.e.* group factors of narrow range. Such factors may operate in the case of the two steadiness tests, or the two match-stick insertion tests, in the motor group, or the two arithmetical tests in the scholastic group.

With a view to obtaining more precise information upon this point, the correlations between the total scores in the mental, scholastic, and character groups were submitted to further investigation by the method of partial correlation.

If we denote

Total score in mental tests by 1,
" " " scholastic tests by 2,
" " " character tests by 3,

then, employing the usual notation for partial coefficients[1], we get the following:

TABLE III. *Values of* $r_{12\cdot3}$ *in various groups.*

Group	Value of $r_{12.3}$
A	·598
B	·543
C	·523
E	·606
	Average = ·568

TABLE IV. *Values of* $r_{23\cdot1}$ *in various groups.*

Group	Value of $r_{13.2}$
A	·187
B	·276
C	·193
E	·317
	Average = ·243

TABLE V. *Values of* $r_{13\cdot2}$ *in various groups.*

Group	Value of $r_{13.2}$
A	·187
B	·070
C	·035
E	– ·026
	Average = ·067

---

[1] See G. Udny Yule, *Phil. Trans.* CXCVIII, A, 443–59.

The following table compares these figures with those given in Table I:

TABLE VI.

Groups of tests correlated	Observed coefficient	Group eliminated	Partial coefficient
Mental and scholastic	·573	Character	·568
Scholastic and character	·335	Mental	·243
Mental and character	·250	Scholastic	·067

(All the above figures are the average correlations for the groups A, B, C and E.)

There are here three facts of importance to be noted:

(1) That when the effect of variations in character is eliminated, the correlation between mental and scholastic ability is practically unaltered.

(2) That when the effect of variations in mental ability is eliminated, the correlation between scholastic ability and character is very slightly lowered.

(3) That when the effect of variations in scholastic ability is eliminated, the correlation between mental ability and character is approximately zero.

The following deductions can be made from the above figures:

(1) That whatever factor (or factors) causes the correlation between mental and scholastic ability, such factor (or factors) does not enter into 'character.'

(2) That whatever factor (or factors) causes the correlation between scholastic ability and character, it is practically independent of mental ability.

(3) That the correlation between mental ability and character is due entirely to the correlation of each with scholastic ability.

We have, then, to look for:

(1) A factor, or group of factors, common to mental and scholastic ability.

(2) A factor, or group of factors, common to scholastic ability and character.

It has also to be remembered that these factors must be distinct, since, when the effect of scholastic ability is eliminated, there remains no correlation between mental ability and character.

V. *Is a 'general factor' present?*

It is now proposed to investigate to what extent the tests within each group exhibit the operation of a general factor. Prof. Spearman has shown that if such a factor is the sole cause of the observed correlations, then the resulting coefficients can be arranged in the hierarchical order[1]. He has recently[2] published an improved 'criterion' by which a table of coefficients can be investigated to see how far they fulfil the conditions for a hierarchy.

This criterion was applied to the tables giving the intercorrelations between the separate tests constituting each group of tests, for each of the groups of subjects. The tables of intercorrelations for Group B, which was numerically the largest group, are given below to illustrate the type of figures obtained. All were calculated by means of the Bravais-Pearson formula. The tables for the other groups are available for reference by anyone interested.

TABLE VII. Group B. 135 *boys*, 12–13. *Mental Tests—Intercorrelations.* (*Decimal points omitted.*)

	Completion	Opposites	Instructions	Number series	Synonyms	Cipher	Absurdities	Analogies
Completion	—	58	58	51	48	43	43	46
Opposites	58	—	47	50	53	40	44	45
Instructions	58	47	—	51	48	41	45	38
Number series	51	50	51	—	34	50	35	40
Synonyms	48	53	48	34	—	33	41	39
Cipher	43	40	41	50	33	—	41	33
Absurdities	43	44	45	35	41	41	—	31
Analogies	46	45	38	40	39	33	31	—

(P.E. varies between ·037 and ·057.)

TABLE VIII. Group B. 135 *boys*, 12–13. *Scholastic Tests—Intercorrelation.* (*Decimal points omitted.*)

	Reading—comprehension	Arithmetic—rules	Dictation	Arithmetic—problems	Reading—speed	Essay	Drawing
Reading—comprehension	—	29	47	25	37	29	20
Arithmetic—rules	29	—	28	55	19	30	22
Dictation	47	28	—	19	43	21	11
Arithmetic—problems	25	55	19	—	10	32	14
Reading—speed	37	19	43	10	—	17	12
Essay	29	30	21	32	17	—	04
Drawing	20	22	11	14	12	04	—

(P.E. varies between ·041 and ·058.)

TABLE IX. Group B. 135 *boys*, 12–13. *Character traits—Intercorrelations.* (*Decimal points omitted.*)

	Conscientiousness	Persistency	Trustworthiness	General character	Leadership	Changeability
Conscientiousness	—	72	77	65	26	-17
Persistency	72	—	68	66	34	-30
Trustworthiness	77	68	—	65	14	-25
General character	65	66	65	—	26	-24
Leadership	26	34	14	26	—	-08
Changeability	-17	-30	-25	-24	-08	—

(P.E. varies between ·024 and ·057.)

---

[1] Hart and Spearman, this *Journal*, 1912, v, 58.

[2] Spearman and Holzinger, this *Journal*, 1924, xv, pt. 1.

TABLE X. Group B. 135 *boys*, 12–13. *Motor Tests—Intercorrelations.* (*Decimal points omitted.*)

	Steadiness, R. hand	Steadiness, L. hand	Matches in box	Match-stick insertion, L. hand	Match-stick insertion, R. hand	Tracing
Steadiness, R. hand	—	69	18	13	12	35
Steadiness, L. hand	69	—	05	08	01	15
Matches in box	18	05	—	28	35	15
Match-stick insertion, L. hand	13	08	28	—	36	09
Match-stick insertion, R. hand	12	01	35	36	—	−06
Tracing	35	15	15	09	−06	—

(P.E. varies between ·030 and ·057.)

Of the tables available, the following were selected for the application of Prof. Spearman's new criterion, because the number of subjects within these groups was sufficient to make the application of the criterion reliable.

(1) *Group B.* (135 boys, ages 12–13.)
  1. Mental tests.
  2. Scholastic tests.
  3. Character traits.
  4. Motor tests.

(2) *Group C.* (61 boys, ages 13–14.)
  1. Mental tests.
  2. Scholastic tests.
  3. Character traits.
  4. Motor tests.

The results for Group B were calculated by the simpler formula given by Spearman and Holzinger[1], and those for Group C were obtained from the table given at the end of their article (by interpolation).

TABLE XI. Group B. *Deviations in intercorrelations from condition for Hierarchy in terms of* P.E. (*Spearman criterion*).

	Between																Outside these limits
	+8 +7 P.E.	+7 +6 P.E.	+6 +5 P.E.	+5 +4 P.E.	+4 +3 P.E.	+3 +2 P.E.	+2 +1 P.E.	+1 0 P.E.	0 −1 P.E.	−1 −2 P.E.	−2 −3 P.E.	−3 −4 P.E.	−4 −5 P.E.	−5 −6 P.E.	−6 −7 P.E.	−7 −8 P.E.	
Mental	0	0	0	1	6	9	27	33	18	22	8	5	3	6	2	0	—
Scholastic	0	3	1	1	1	7	10	8	20	12	4	7	3	0	2	3	2
Motor	2	1	0	0	2	1	2	9	10	3	3	3	3	0	1	2	1
Character	0	1	0	0	3	5	6	5	3	6	4	1	3	4	1	0	3

---

[1] See Spearman and Holzinger, *op. cit.* p. 13.

These figures can be summarized as follows:

Table XII. (*Summary of Table XI.*)

	Between									
	±P.E.		±2 P.E.		±3 P.E.		±4 P.E.		±5 P.E.	
	No.	%	No.	%	No.	%	No.	%	No.	%
Mental	51	36·4	100	71·4	117	83·6	128	91·4	132	94·3
Scholastic	28	33·3	50	59·5	61	72·6	69	82·1	73	86·9
Motor	19	42·2	24	53·3	28	62·2	33	73·3	36	80·0
Character	8	17·8	20	44·4	29	64·6	33	73·3	36	80·0

The corresponding figures for Group C are:

Table XIII. (*As Table XI, Group C.*)

	Between																Out side these limits
	+8 +7 P.E.	+7 +6 P.E.	+6 +5 P.E.	+5 +4 P.E.	+4 +3 P.E.	+3 +2 P.E.	+2 +1 P.E.	+1 0 P.E.	0 −1 P.E.	−1 −2 P.E.	−2 −3 P.E.	−3 −4 P.E.	−4 −5 P.E.	−5 −6 P.E.	−6 −7 P.E.	−7 −8 P.E.	
Mental	0	0	0	4	2	9	21	32	20	15	20	11	2	2	2	0	—
Scholastic	0	0	3	3	7	5	11	13	12	7	10	8	1	3	1	0	—
Motor	0	1	2	1	1	1	7	3	7	8	9	2	3	0	0	0	—
Character	0	0	0	2	3	5	3	5	6	10	2	9	0	0	0	0	—

Table XIV. (*As Table XII, Group C.*)

	Between									
	±P.E.		±2 P.E.		±3 P.E.		±4 P.E.		±5 P.E.	
	No.	%	No.	%	No.	%	No.	%	No.	%
Mental	52	37·1	88	62·9	117	83·6	130	92·8	136	97·1
Scholastic	25	29·8	43	51·2	58	69·0	73	86·9	77	91·7
Motor	10	22·2	25	55·6	35	77·8	38	84·4	42	93·3
Character	11	23·4	24	53·5	31	68·9	43	95·6	45	100·0

The question now is to what extent any of these figures can be claimed to indicate the operation of a single factor. For a normal distribution the percentages should be:

Between ± P.E.	50·0 per cent.
± 2 P.E.	82·3
± 3 P.E.	95·7
± 4 P.E.	99·3
± 5 P.E.	99·9

The following table enables comparisons to be made easily:

TABLE XV. (*All figures are percentages.*) (*Comparing figures in Tables XII and XIV with normal distributions.*)

		±P.E.	±2 P.E.	±3 P.E.	±4 P.E.	±5 P.E.
Normal distribution		50·0	82·3	95·7	99·3	99·9
Mental tests	B	36·4	71·4	83·6	91·4	94·3
	C	37·1	62·9	83·6	92·8	97·1
Scholastic tests	B	33·3	59·5	72·6	82·1	86·9
	C	29·8	51·2	69·0	86·9	91·7
Character traits	B	17·8	44·4	64·6	73·3	80·0
	C	23·4	53·5	68·9	95·6	100·0
Motor tests	B	42·2	53·3	62·2	73·3	80·0
	C	22·2	55·6	77·8	84·4	93·3

Prof. Spearman has suggested that the deviations of these figures from those demanded theoretically by the "Theory of Two Factors" may be due to either, or both, of two causes.

(1) To the abbreviated formula used for the calculation of the P.E. In place of that used above he suggested the use of the following formula:

$$(\text{P.E.})^2 = \frac{(1{\cdot}349)^2}{N}(p^2 + s^2),$$

where $p = r(1 - r)$ and r is the mean value of the four correlation coefficients forming the 'tetrad,' and s^2 = mean square deviation of the same four coefficients.

The change is the introduction of the s^2 term, and this may be considerable when the dispersion of the coefficients is wide. If the number of cases is not large it is necessary to include the term $+\frac{1}{2}\frac{(1-r^2)^4}{N}$ in the bracket in addition, r being as above.

(2) Specific correlation. This may apply in the case of some of the motor tests, such as the two steadiness tests, or the two match-stick insertion tests, or between some of the scholastic tests, such as the two arithmetic tests.

The above formula has been applied to the data obtained during the present research. For Group B (135 cases) the form

$$(\text{P.E.})^2 = \frac{(1{\cdot}349)^2}{N}(p^2 + s^2)$$

was employed and for Group C (61 cases) the full form was used. The following results were obtained:

TABLE XVI. (*All figures are percentages.*) (*As Table XV, but using more accurate formula for the* P.E.)

		±P.E.	±2 P.E.	±3 P.E.	±4 P.E.	±5 P.E.
Normal distribution		50·0	82·3	95·7	99·3	99·9
Mental tests	B	43·0	81·0	96·0	99·2	100·0
	C	44·0	67·0	84·0	96·0	100·0
Scholastic tests	B	38·1	63·1	77·4	82·3	86·9
	C	32·1	57·1	79·8	97·6	100·0
Character traits	B	28·9	60·0	84·4	93·3	100·0
	C	64·4	93·3	100·0	100·0	100·0
Motor tests	B	46·7	60·0	80·0	80·0	88·9
	C	35·6	73·3	84·4	93·3	97·8

VI. *Conclusions as to a 'general factor.'*

On the basis of the above figures[1] certain conclusions can be drawn.

(1) *The Mental Tests.* There is clearly a general factor operative here. The percentages for Group B (which it should be remembered contained the largest number of subjects and is therefore the most reliable) are in very close agreement with the requirements of theory.

(2) *The Scholastic Tests.* The figures here show less general agreement with the requirements of theory. This may be due in part to 'specific correlation' due to the tests not fulfilling the requirement of being 'sufficiently dissimilar.' It is at any rate clear that there is a factor (or factors) other than the general factor at work, and it should be noticed that there is a slight tendency for the figures in the older group (C) to be in closer agreement with the theory than those for the younger one (B).

(3) *The Character Ratings.* Again the demands of the theory are not fully met, but here again the figures for the older group (C) are in closer agreement with it, than those for the younger one (B). It must be remembered, however, as already pointed out, that the figures here are based on 'ratings,' and cannot, therefore, be claimed to be of such reliability as those based upon scores made in actual tests.

(4) *The Motor Tests.* Here the figures are not in close agreement with the demands of the theory. The disturbing element may be the 'specific correlation' noted above.

---

[1] Summarized in an alternative way, these figures give the following values for the ratio $\frac{\text{Actual P.E.}}{\text{Theoretical P.E.}}$ over the whole table:

	Group B	Group C
Mental tests	1·040	1·079
Scholastic tests	1·577	1·191
Character ratings	1·496	0·756
Motor tests	1·577	1·350

Combining these facts with those obtained from the figures obtained by the method of partial correlation given above, there are indications of the operation of the following factors:

(1) A general factor operative in the mental tests. The other groups of tests do not fulfil the requirements of the 'two factor' theory. This may be due to similarity between certain tests, but it should be noted that there is a tendency for the figures for the older group to be more closely in keeping with the theory than those for the younger one.

(2) This may indicate another factor of wide significance which becomes more general as the age of the subjects increases. It is difficult to resist the surmise that the first of these factors is innate and the other acquired. This gives a hint as to the possible nature of the factors involved, but before proceeding to discuss this point, a very brief summary of work already carried out, and which bears upon this, would not be out of place.

Spearman[1], in one of the papers to which reference has been made, gave his reasons for considering that his general factor in intelligence was "a central fund of intellective energy."

Dr Burt[2], while agreeing that the results of a series of tests pointed to the operation of a general factor, held that this factor was the power of 'voluntary' attention.

Dr Webb[3], using as subjects mainly Training College students of average age 21 years, found the presence of a general factor which he named "persistence of motives," adding that by this conception he understood consistency of action resulting from deliberate volition or will. Dr Maxwell Garnett[4], working with Webb's material, discovered another factor of wide significance which he termed 'cleverness' and defined as the "tendency to associate by similarity," while he renamed Webb's persistency of motives 'purposefulness.'

Meanwhile Dr Burt[5] had published evidence for there being a central emotional factor and states[6] that there appears to be evidence for:

(1) A general intellectual factor, which is innate.

(2) A central emotional factor, which is also innate.

(3) What looks like a central moral factor, which is, probably, in the main, acquired.

---

[1] Spearman and Hart, "General Ability; its existence and nature," this *Journal*, 1912, v.

[2] Burt, "Experimental Tests of General Intelligence, this *Journal*, 1909–10, III.

[3] Webb, *op. cit.*

[4] Maxwell Garnett, "General Ability, Cleverness and Purpose," this *Journal*, 1919, IX

[5] British Association Report, Manchester Meeting, 1915.

[6] In a letter to the present writer.

This analysis should be compared with that of Dr Maxwell Garnett quoted above. He maintains that up to the present three factors of wide generality have been found, viz.:

(1) 'g'—general intelligence—the power of voluntarily concentrating the will.

(2) 'c'—cleverness—the power of associating by similarity.

(3) 'p'—or purpose, giving rise to 'consistency of motives.'

How far, then, do any of the above researches agree with the indications of the figures quoted in this one? And is it possible to develop the factors required from Spearman's conception of the general factor as "a central fund of intellective energy"?

There are two possible views of the development of intelligence. Either (1) intelligent activities have developed from activities based upon the instincts or (2) intelligent activities have developed independently. The main facts seem to point to (1) as being the true view, and this point of view will be assumed in what follows. Now the mark of a purely instinctive act is its "all—or none" character. On the other hand, the mark of an intelligent act is its 'graded' or controlled nature. This is the first point which it is desired to emphasize. It is suggested that Prof. Spearman's "central fund of intellective" energy is not the whole of the energy which is at the disposal of the individual, but only that portion of it which he has 'graded' or under control. Many individuals who would be classed as of low intelligence have certain instincts strongly developed. Another consideration now arises. Just as individuals may vary either (1) in the total energy at their disposal or (2) in the amount of this energy which they have graded or controlled, so too it would seem possible that they may differ in the firmness of this control. This would give, then, three factors.

(1) The *total energy* at the disposal of the individual, *i.e.* the strength of the instincts forming the bases of his activities.

(2) The *amount* of *graded energy* at his disposal.

(3) The *firmness* of the *control, or grading* of the energy under (2).

Some indication has been found above that there was a factor in mental ability which appeared to be fairly constant with age, and the possibility of one in character which exercised more effect, the older the subjects. (In this respect it is interesting to note that Webb's work was carried out mainly with subjects of average age 21 years.) It is suggested that these factors are (2) and (3) respectively. Such factors would satisfy all the requirements which have arisen in the course of this work. Scholastic and mental tests correlate because they both involve (2) and this factor is probably innate. Scholastic tests and character traits correlate because they both involve (3) and this factor is clearly one which can increase in its effect with age. Mental tests and character traits correlate because they both correlate with scholastic tests. This would indicate:

(1) That success in mental tests depends in the main on one factor.

(2) That success in scholastic tests depends upon at least two factors of wide generality. This last point is not without significance in dealing with the question as to how far mental tests can be used in place of scholastic examinations.

These factors may be usefully compared with those discussed by Dr Burt and quoted above. Dr Burt's three factors are:

(1) A central emotional factor.

(2) A general intellectual factor.

(3) A central moral factor.

Writing with reference to the nature of these three factors, Dr Burt states[1] that he would identify (1) with "the total energy at the disposal of the individual"; that (2) is not "an energy at all, but a way in which energy is used," while (3) corresponds to the "firmness of the grading or control." He adds: "It seems to me to depend largely on the individual's power of organizing his emotions into stable sentiments; and thus...it may be intimately associated with the child's power of learning at school. To some extent it varies directly with the intellectual factor and inversely with the emotional factor, and certainly does not seem quite so independent of these two as these two are of each other, nor yet so preponderantly innate."

The present research does not agree with the whole of this last sentence, but the difficulty may be one mainly due to the fact that the measurement of moral qualities is a matter of extreme difficulty.

The figures given in the present paper seem to indicate that the character qualities measured are practically independent of mental ability. It is clear that neither statement is true at the extreme ends of the scale of mental ability. It is hardly to be expected that one would find a mentally deficient individual of high moral character, but neither does one always find the individual of brilliant intellectual power showing the soundest moral character. Much evidence of a biographical nature might be quoted in support of this statement. Among instances quoted by Dr Burt, and illustrating it, are the following criminals of genius: Benvenuto Cellini, statuary, sonneteer, the greatest goldsmith of his time, and autobiographer of his own misdeeds; Villon, poet and thief; Eugene Aram, philologist and murderer; Vidocq, burglar and detective; Prof. Webster, a chemist who used his laboratory to dispose of the bodies of his victims; and Wainewright, art critic and collector, who poisoned his insured relatives to pay for his gems and antiques. But these are exceptions, and, outside fiction, the majority of criminals are dull and unstable rather than supernormal.

---

[1] In a letter to the present writer.

Dr Maxwell Garnett's threefold scheme is somewhat similar to Dr Burt's. His three factors are:

(1) Power of voluntary attention.
(2) Purposefulness.
(3) Cleverness.

(1) is the general factor in intelligence, and this Garnett, agreeing with Burt, conceives to be a *power*, rather than a quantity. (2) corresponds to Burt's central moral factor and is "will at long range," the ability to continue a definite line of action, and this again may be compared with the character factor described above, viz. the firmness of the grading or control. (3) is a factor which, as Garnett defines it, depends to some considerable extent upon emotionality. The person who possesses it may not necessarily achieve much and this point should be compared with the discussion on types given below.

It will be seen that there is general agreement on the nature of the character factor and on the fact that there is an instinctive (or emotional) factor. The divergence comes in the nature of the general intellectual factor and the present work adds very little to the discussion which has already taken place on this point. On grounds apart from statistical work it would seem that a case could be made out for Spearman's view of it as a "central fund of energy" and this conception is retained here. Dr Burt holds that "intelligence is a matter of brain structure rather than of brain energy" and that "energy is a matter of instinct or emotion rather than of intelligence." But even if intelligence is a way in which energy is used, the *amount* of energy available for use must control to a very large extent the degree of intelligence shown and must therefore appear as a general factor in intelligence. But the discussion on this point must clearly await the development of some crucial test.

VII. *Three significant factors in mental ability.*

There are, then, it would appear, at least three factors of wide significance in mental ability. These have been already stated on p. 214 above. The effect of these three factors can be best seen by tabulating the extreme possibilities that can occur. Denoting the possession of a factor to a degree above the normal by + and to a degree below normal by −, the following table is obtained:

TABLE XVII.

Types	1	2	3	4	5	6	7	8
Total energy (emotional factor)	+	+	+	+	−	−	−	−
Graded energy (intellectual factor)	+	+	−	−	+	+	−	−
Firmness of grading (moral factor)	+	−	−	+	+	−	−	+

The above are the theoretical possibilities. It is not suggested that these are rigidly distinct classes. Each class tends to overlap some other classes and many individuals (if not most) are 'intermediate.' But the tendency towards one or other of the above classes can often be seen in pupils in a school. (Some individual cases are given later.) Appended is a brief description of the various classes tabulated above.

Class 1. This is the individual of high native ability, and energy who is also well-disciplined. He ranks high both in intelligence tests and in school ability.

Class 2. This is the individual of high native ability, but who is lacking in character. He does not concentrate—his teachers rank him as 'idle but able' or even (erroneously) as 'unintelligent' according to his ability to disguise his real powers. In intelligence tests he does well, but he ranks much lower when rated by scholastic tests or the estimates of his teachers. Some individuals in this class form the non-defective delinquents, and this class includes the intelligent but 'unstable' child.

Class 3. The 'animal' man—unable to control in any way the energy of his instincts, and therefore lacking in intelligence. This class furnishes many defective delinquents, and includes the defective and 'unstable' child.

Class 4. Subjects of defective intelligence but reliable and energetic within their capabilities. They are emotional and may be expected to show strong likings or antipathies without any necessary grounds for them.

Class 5. This is the individual of poorer native ability, but conscientious and industrious. What energy he has graded is well controlled. When tested by intelligence tests he ranks lower than individuals belonging to the previous three classes, but he frequently ranks above subjects included in Classes 2 and 3 when tested by scholastic examinations or when rated by his teachers.

Class 6. The individual on a par with those of the previous class as regards ability but lacking in application. Intelligence tests and scholastic tests agree in ranking him in a low position.

Class 7. This, in the extreme case, is the apathetic defective, perhaps the commonest and most typical case of mental deficiency.

Class 8. The subjects included here are defective in intelligence and emotionality but 'dependable' within their limits. They are 'sluggish' and hard to interest in any way. This class furnishes the 'stable defectives.'

These possibilities may be illustrated graphically in another way.

Assuming normal distribution for each of the factors involved, we get the following diagram:

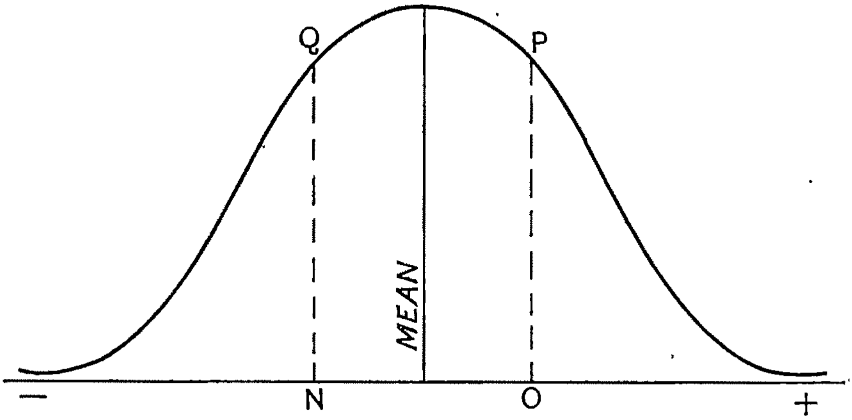


Fig. 2. Distribution of Total Energy (Emotional Factor).

Now consider the subjects along any section to the right of the mean, say along *OP*. These would be cases marked + for this factor in Table XVII. Similarly the subjects along *NQ* would be among those marked. Now the subjects along *OP* or *NQ* need not have the same amount of energy 'graded,' or controlled, and a distribution of these subjects with regard to this factor would give the following diagrams:

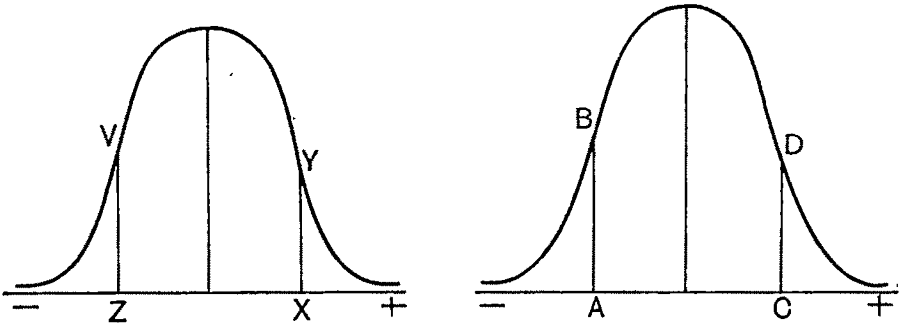


Fig. 3. Distribution of cases along *NQ* with reference to 'graded' energy.

Fig. 4. Same for cases along *OP*.

Any case along VZ would be $-$ for both graded energy and for total energy; one along YX would be $+$ for graded energy, but $-$ for total energy, while one along AB would be $-$ for graded energy but $+$ for total energy, and one along CD would be $+$ for both. Now the total cases along either VZ, YX, BA or DC could be arranged to show the distribution with regard to the third factor—firmness of control—and this would give two extremes along each of the sections VZ, YX, BA and DC. This would give the eight types shown in Table XVII.

VIII. *Some individual cases.*

The following cases are cited as examples of some of the above classes. They are all boys—students at a secondary school—and have been known to the writer for a considerable period. The intelligence tests employed were the group of mental tests described above.

Case 1. Chronological age, 14·6. Tested by intelligence tests he is easily first in a group of 18 boys of 14–15 years. He has always been top of his form in terminal examinations for the last three years and has recently won an open scholarship at a well-known Public School.

Case 2. Chronological age, 13·1. Ranking by intelligence tests is second in a group of 17 boys of 13–14 years and third in a group of 35 boys of 13–15 years. He is, by scholastic examinations, third in a form of 21 boys. Cases 1 and 2 are illustrative of Class 1 (above).

Case 3. Chronological age, 13·5. Tested by the intelligence tests he is top of a group of 17 boys of 13–14 years, and second in a group of 35 boys from 13–15 years. (Case 1 was the only boy who scored higher.) As the result of terminal examinations he has always been in the lower half of his form—at his last school examination he was eighth in a form of 11 boys.

Case 4. Chronological age, 14·9. By the intelligence tests he is second in a group of 18 boys from 14–15 years. He has been in the bottom quarter of his form throughout his school life and is now bottom of his form. Cases 3 and 4 illustrate Class 2 (above).

Case 5. Chronological age, 11·3. By intelligence tests he is fifth in a group of 35 boys. In his form he is fourteenth out of 20 boys and he has already a bad record as regards conduct. This case illustrates the members of Class 2 (above) who become delinquents.

Case 6. Chronological age, 13·10. By the intelligence tests he is ninth in a group of 17 boys from 13–14 years and twentieth in a group of 35 from 13–15. He has always been top of his form for the past three years and is regarded by his fellow pupils as the natural boy to be head of the form.

Case 7. Chronological age, 13·10. By the intelligence tests he is fourteenth in a group of 17 boys from 13–14 years and twenty-ninth in a group of 35 from 13–15 years. He entered the school with a scholarship, is now in a form higher than any other boy of his chronological age group and is doing School Certificate work. Cases 6 and 7 illustrate Class 5 (above).

Case 8. Chronological age, 13·11. By the intelligence tests he is bottom of the group of 17 boys from 13–14 years and thirty-fourth in the group of 35 boys from 13–15 years. He is eighteenth in his form (21 boys), and illustrates Class 6 (above).

Case 9. Chronological age, 14·3. By the intelligence tests he is bottom of both age groups: the average score for his group in these tests was 160, with a standard deviation of 27. His actual score was 60. He is in the lowest form in the school and in the lower half of this form. He illustrates Class 7 (above).

If a group of subjects were tested by a group test of intelligence and also for scholastic ability, the probability is that the rankings would be as follows:

High in both tests	High in intelligence tests, lower in scholastic	Low in intelligence tests, higher in scholastic	Low in both
Class 1	Classes 2 and 6	Classes 4, 5 and 8	Classes 3 and 7

That is, subjects belonging to three of the classes would tend to be ranked similarly by both tests, while those belonging to the other five classes would tend to be ranked differently by the two tests. It is suggested that herein lies the cause of the imperfect correlation which has been demonstrated to exist between the two methods of testing pupils. Of course there are other disturbing factors, but the main point is that intelligence tests measure to some degree the "fund of intellective energy" at the disposal of the subject, while the scholastic tests measure the firmness of his control over this fund, the extent to which he can apply it *continuously*.

The theory outlined above also serves to explain the fact that the correlation between the results of intelligence and scholastic tests varies in different groups. For the proportion of subjects in the various groups will vary from sample to sample. If the larger proportion of subjects be within Classes 1, 3 or 7 the correlation will be high, while if the larger proportion are within Classes 2, 4, 5, 6, or 8 it will be low.

IX. *Summary.*

The points dealt with above may now be summarized very briefly. They are:

(1) There is definite evidence of a general factor operative in intelligence tests.

(2) There is some indication of a general factor operative in character traits in young children, and also some evidence that such a factor develops, and becomes increasingly important, as the subjects grow older.

(3) There appears to be some ground for holding that scholastic ability depends on *both* these factors, which are independent of one another.

(4) Intelligence tests may select pupils of two distinct types:

- (*a*) Those of high intelligence and of high powers of application (+ character factor).
- (*b*) Those of high intelligence but of small powers of application (− character factor).

(5) The two types detailed under 4 (*a*) and 4 (*b*) clearly require (in the main) entirely different treatment if they are to fulfil their promise and the appropriate method in each case is a point requiring further research.

I am much indebted to Dr C. Burt for many very helpful notes on the subject of this paper. To Mr W. J. Webster my thanks are due for his great assistance in the work of actually administering the various tests. I also wish to express my thanks to Prof. Spearman for suggesting the use of an improved formula for the calculation of the Probable Error and for other valuable suggestions.

(*Manuscript received* 19 *September*, 1925.)

ON CERTAIN EFFECTS OF LONG SPELLS OF REPETITIVE WORK

By M. D. VERNON.

(From the Cambridge Psychological Laboratory.)

I. *Introduction* (p. 222).
II. *Experimental work of B. Muscio* (pp. 222, 223).
III. *Experimental work of Dr G. Teruoka* (pp. 223, 224).
IV. *Further work on the Pendulum Test* (p. 224).
V. *Reasons for Adoption of New Tests* (pp. 224, 225).
VI. *Description of Paper-folding Test* (pp. 225, 226).
VII. *Description of Maze-tracing Test* (pp. 226, 227).
VIII. *Results of the two Tests* (pp. 227–229).
IX. *Subjective Experience* (pp. 229–232).
X. *Comparison of the Results of the Different Tests* (pp. 232, 233).
XI. *Observations on the Industrial Application of Results* (pp. 233–235).
XII. *Summary and Conclusion* (pp. 235, 236).

I.

Much experimental research has recently been carried out on work which does not call for excessive muscular effort, but is of a repetitive nature. The investigations which will be described in this paper deal with attempts which have been made to determine whether any signs of fatigue, subjective or objective, are likely to occur in long spells of such work. The operations employed have been of a simple nature; and a continuous succession of such operations has been carried out over periods of time of considerable, though variable, length. The amount of the output, together with the accuracy of the performance, have been measured, and their variations throughout the spells of work recorded; and the conclusions to be drawn from these measurements are discussed below.

II.

An investigation "On the Relation of Fatigue and Accuracy to Speed and Duration of Work[1]" was carried out by B. Muscio in 1922. Muscio employed an "Aiming Test," which consists essentially "in spearing at targets with a dissecting needle (mounted on a wooden handle)," and a "Pendulum Test" carried out with Dr W. R. Miles' Pursuit Pendulum[1].

---

[1] Reports of the Industrial Fatigue Research Board, No. 19. (General Series, No. 7.)

Muscio did four long spells of continuous work, he himself acting as subject in each case. These four spells consisted of:

(1) A spell of 2 hrs. 20 mins. at the Aiming Test. The rate of work was alternately increased and decreased at a fairly rapid rate.

(2) A spell of 2½ hrs. at the Pendulum Test.

(3) A spell of 3½ hrs. at the Pendulum Test.

(4) A spell of 2 hrs. at the Aiming Test. In this, the rate of work was gradually increased throughout, the increase taking place in eight successive steps. This increase from first to last was about 14 per cent.

In the first three of these spells he obtained a gradual and continuous increase of accuracy up to the end of the third hour. In the fourth spell, accuracy increased for about three-quarters of an hour and after that decreased fairly steadily; the decrease appears to have been about 60 per cent.

From these experimental data Muscio concluded that inaccuracy in this type of work was a function of the rate of work rather than of the duration of work.

III.

A further investigation on the Aiming Test was carried out by Dr G. Teruoka of Kurashiki, Bitchu, Japan, in 1923. Dr Teruoka worked at the Cambridge Psychological Laboratory, England. Details of his work have not hitherto been published in English. On each of two successive days he carried out at the Aiming Test two spells of continuous work of three and a half hours' duration, one in the morning, and one in the afternoon. He also carried out a three and a half hours' continuous spell with gradual increase of rate of work, similar to Muscio's two hour spell; and in addition a three and a half hours' continuous spell with a decreasing instead of an increasing rate.

In the two former experiments results were obtained which agreed mutually but disagreed with those of Muscio. In Teruoka's experiments accuracy was found to increase continuously in the morning spells up to the end of the second hour; after that it gradually decreased. In the afternoon spells, accuracy increased up to the middle of the second hour, *i.e.* for an hour and a half, and after that decreased.

---

[1] *The Psychological Review*, XXVII, pp. 361–367, 1920.

In the latter experiments he obtained, with increased rate, a decrease of accuracy similar to that of Muscio. But with decreased rate, although there was throughout a general increase of accuracy, in the latter half of the spell there was usually a decrease of accuracy towards the end of the period at each particular rate; *i.e.* the accuracy at a particular rate tended to decrease, though it would increase as soon as the rate was lowered. This tends to show that in this test and for this subject fatigue was operative during the latter part of a long spell not only if the rate of work was kept constant but even when it was gradually decreased. These results, therefore, do not bear out those of Muscio.

IV.

Some work was done by the present writer with the Pendulum Test, to discover whether similar or divergent results would be obtained from it with a different subject. In addition to a large number of shorter spells, four spells of three hours' duration were carried out; one of these being followed, after an hour's interval, by a further spell of two hours. The results were not very definite, except in so far that they in no way bore out those of Muscio. A slight increase of accuracy usually occurred at the beginning of the spell; but the maximum accuracy was always obtained before the end of the first hour. After that, a gradual diminution of accuracy took place till about the end of the second hour; after this, the accuracy curve remained level or rose very slightly.

It should be said that owing to various instrumental and other causes of error, no very great reliance could be placed in the results. But there was little doubt that they were substantially different from those of Muscio.

V.

As the Aiming and Pendulum Tests did not seem to be altogether satisfactory for this type of investigation, it was decided to employ a new test in which the movements were more complex than those involved in the Aiming Test, and less muscularly fatiguing than those of the Pendulum Test. The essentially repetitive character of the work was retained; but the individual operations were rendered somewhat more complex, since it was desired to experiment with a type of work in which a certain amount of voluntary attention might be necessary throughout.

In the Aiming and Pendulum Tests no variation of rate of work was possible during the actual operation. It seemed quite likely that interesting effects might be obtained if no definite rate of work were prescribed, but the subject allowed, within limits, to set his own rate. This plan was therefore adopted, with the provision that the rate of work should never fall below a certain minimum, since otherwise no inaccuracies of appreciable magnitude would be likely to occur.

Finally, it was desired to measure not only the general trend of the performance but also the individual values. In the Aiming Test a 'unit' consisted of at least 30 shots; and in the Pendulum Test a natural unit really consisted of 20 catches, for a pause was made after 20 catches. It was decided to time, measure and record each individual performance in order that the significance of outstanding individual performances might, if necessary, be noted and stressed.

In compliance with these conditions, two new tests were devised. The first was not found to be altogether satisfactory, partly because of the necessity of an observer to watch and record the progress of the test; and partly because of the labour involved in the subsequent measurements and calculations. It is, however, described below, for a certain number of results were obtained from it. Later a second test was substituted, the results of which were mechanically recorded and were capable of ready evaluation.

VI.

The first test consisted essentially of the folding of square pieces of paper in a certain pre-arranged pattern, and then cutting a portion from the folded paper with a photographic print cutter. Five different sets or types of folds were employed; and these types followed one another in a fixed order. A system of numbers was printed on each paper form, and the subject was required to fold each form first from 1 to 1, then from 2 to 2, and so on (see Fig. 1). Finally he placed the folded form on the cutter and cut along the straight line printed on the form, placing the result of his work at his side. When he had folded and cut the five different forms, he repeated them in the same order, and continued until the experimental 'spell' was completed.

An experimenter sat with the subject and recorded the time for folding and cutting of each form; the time being measured from the cutting of one form to the cutting of the next. He also numbered the forms in order 1, 2, 3, etc. as they were completed, and placed them on one side.

After the conclusion of the experiment, the cut forms were opened out and the error produced by inaccurate folding and cutting measured. The forms were all folded and cut in such a manner that a piece of a certain fixed shape was cut out of the middle of the form. The shapes for the five different types of form were as follows:

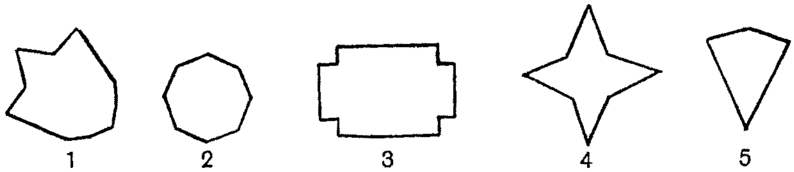


Fig. 1.

The error of the performance was measured by placing the form beneath a transparent sheet of talc upon which the accurate shape had been marked, and recording the maximum linear divergence between the two shapes.

Two subjects worked at the experiment. They will be called *A* and *B* respectively. Both were adult, one (*A*) being male, and one (*B*) female. After several practice trials, two long spells of four hours' duration were carried out by Subject *A* and three by Subject *B*. In addition, *B* carried out one period of eight hours, a spell of four hours in the morning followed, after an hour's interval, by a spell of four hours in the afternoon. Work was continuous throughout the spells, and there were no rest pauses.

VII.

In the second test, which was substituted for the Paper-folding Test, the same principles were observed although the actual operations performed were of a different type. Five metal 'mazes,' all of different patterns, of the kind employed for tests of manual dexterity, were screwed to a board covered with insulating material. The mazes were connected in series with the pointer of a recording drum, an accumulator and a metal stiletto. The subject was required to trace the path of the maze so as to avoid contact with the metal, all contacts being automatically recorded. In addition, a narrow metal strip was screwed to the insulating board and connected up with a second recorder, in parallel to the circuit through the mazes. The subject traced each of the five mazes in turn, and between each was required to touch the metal strip, in order to give a time signal for the commencement of each maze. A time marker also traced upon the drum. Hence subsequently the time taken to trace each maze, and the number of 'touches' or errors made, could be tabulated. The mazes were set at an angle of about 45° with the vertical; and the tracing arm was supported upon a table.

This test was performed by one subject only, namely Subject *B*. After a number of practice trials, in which the sequence of five mazes was repeated continuously for periods of one to four hours, a week's work of 44 hours was undertaken. A four-hour spell was worked for six mornings of the week from 9 a.m. till 1 p.m. and from 2 p.m. till 6 p.m. in the afternoons of five days, there being no afternoon spell on Saturday. During this week the work was not absolutely continuous throughout the spell. A pause of about five minutes occurred half-way through each spell; and in the afternoon pause a cup of tea was taken. In addition it was necessary to pause for about one minute at the end of every twenty minutes in order to replenish the recorders with ink.

VIII.

The results of both experiments were averaged in periods of one hour, and recorded as a percentage of the mean of all values for the experiment. In Table I are given the percentage hourly averages of output and extent of errors for the Paper-folding experiments. The output values were obtained from the reciprocals of the times of folding. In Table II are given similar values for the Maze-tracing Test; but here the number of errors are averaged, not the extent.

In Table II the results for Saturday are omitted; firstly, because considerable machine trouble caused a break during the first hour, and, secondly, because there appeared to be a large end-spurt due to the approaching close of the experiment.

With the Paper-folding Test it appears that in the case of Subject *A* practice effects had not all worn off when the experiment began. His

TABLE I.

Paper-folding Test.

	Rate of output*				Errors*			
	1st hr.	2nd hr.	3rd hr.	4th hr.	1st hr.	2nd hr.	3rd hr.	4th hr.
Subject *A*								
4 hrs. (1)	83·8	97·3	105·8	116·8	92·7	84·3	101·5	121·0
4 hrs. (2)	91·8	95·7	100·4	110·0	94·3	111·2	97·1	105·5
Subject *B*								
4 hrs. (1)	95·3	100·0	102·1	103·0	101·1	94·6	101·1	98·1
4 hrs. (2)	98·1	99·9	99·9	102·0	100·8	102·5	95·6	101·4
4 hrs. (3)	97·4	101·5	100·2	101·4	98·8	101·2	103·7	96·3
8 hrs. (morning)	100·7	101·5	99·8	104·2	104·3	112·7	98·0	96·0
8 hrs. (afternoon)	102·9	98·6	97·7	95·9	87·6	98·1	99·0	104·0

* The rate of output and number or extent of errors are expressed as percentages of the average rates for each subject throughout the experiment.

Table II.

Maze-tracing.

	Rate of output*								
	Morning				Afternoon				Daily av.
	1st hr.	2nd hr.	3rd hr.	4th hr.	1st hr.	2nd hr.	3rd hr.	4th hr.	
Monday	102·9	106·3	106·3	102·9	99·7	96·7	96·7	99·7	101·4
Tuesday	96·7	102·9	106·3	99·7	96·7	99·7	99·7	91·2	96·6
Wednesday	93·8	99·7	93·8	96·7	96·7	99·7	96·7	102·9	97·5
Thursday	99·7	106·3	102·9	102·9	96·7	99·7	96·7	99·7	100·6
Friday	99·7	102·9	102·9	102·9	102·9	99·7	99·7	102·9	101·7
Week's av.	98·5	103·6	102·3	101·0	98·5	99·2	97·9	99·2	
	No. of errors*								
Monday	96·8	109·7	96·8	116·1	103·3	109·7	103·3	116·1	106·5
Tuesday	109·7	90·3	71·0	77·4	116·1	122·6	109·7	122·6	102·4
Wednesday	103·3	83·9	96·8	109·7	90·3	77·4	83·9	103·3	93·6
Thursday	77·4	64·5	90·3	96·8	116·1	109·7	103·3	129·0	98·4
Friday	83·9	83·9	77·4	90·3	148·4	122·6	90·3	103·3	100·0
Week's av.	94·2	86·5	86·5	98·4	114·9	108·4	98·4	114·9	

* The rate of output and number of errors are expressed as percentages of the average rates for the subject throughout the experiment.

output increases rapidly throughout. Subject *B* also increases very slightly, but all practice effects seem to have disappeared at any rate by the eight-hour experiment. In the latter a decrease of output of about 16 per cent. can be observed in the afternoon spell. Errors are also decreased, though only by 4½ per cent. The decrease of output may be due to fatigue, or merely to the fact that work was more careful and slower in the afternoon than in the morning.

In the Maze-tracing Test the hourly average output values show no very marked general tendencies. The effects of practice have not been completely eliminated. There is a gradual rise of output from Tuesday till Friday. It is, however, very slight. The week's averages show that output is a maximum during the second hour of work; and that in the afternoon it is almost level, but is 2·7 per cent. lower than in the morning. Except on Wednesday the errors are uniformly higher in the afternoon than in the morning.

Thus it appears from the averages that the results are negative rather than positive. There is some variation among the individual results, but no very general tendency is shown. There is a slight decrease of output in the afternoons in both tests, and in the Maze-tracing Test an increase of errors; this might be due to fatigue. But the effect is not carried over from day to day, for the daily output attains its maximum on Friday and the daily errors their minimum on Wednesday.

It was noted, however, during the work that at certain periods of the day the subject's performance became very variable. This fact is not usually indicated in the averages, which of course do not show individual variations. It does appear if the measures of standard deviation are inspected. But the outstanding feature of the variability which occurred in both of the tests was that the bad and good performances were grouped separately, bad groups alternating with good groups. In order to show this grouping of performances, the following procedure was adopted. On examining the record it was found that the bulk of the values lay near together; that is to say, they deviated but little from the mean. But it was possible to note by inspection that there were a few, lying outside this central group, which showed an unusually bad or good performance, either as regards time or errors. These outstanding performances were then marked in the record. No very exact criterion was adopted in selecting them, for the object was simply to indicate the grouping of bad and good performances. The deviations of these good and bad values from the mean of the day's performance lay, in general, within a range of ± 1·0 to ± 1·5 times the average standard deviation from that mean. Thus, assuming the values to be distributed normally, about 25 to 30 per cent. of all values were good or bad values. The number of these values in each five mazes traced or five forms folded was then plotted in a curve. (The maximum number of good and bad values for each group of five is ten, one time value and one errors value for each maze or form.) In the accompanying curves (see Figs. 2 and 3) samples of such variability are shown. The curves for Tuesday morning, 2nd hour, in the Maze-tracing, are those of a period of low variability; the small number of good and bad values are not grouped periodically, but occur promiscuously. It may be contrasted with the other curves for Maze-tracing and Paper-folding, which show how the good and bad values are grouped in alternating periods, though with some overlapping. These are, of course, only samples of such periods of variability. The same phenomena appear on other occasions.

IX.

One of the chief interests of these experiments was the possibility of obtaining introspective data. A record was kept for both tests, and these subjective experiences may profitably be compared with the objective results.

In the first place, Subject *B* was able to carry out the tests for the greater part of the working period with an almost complete absence of

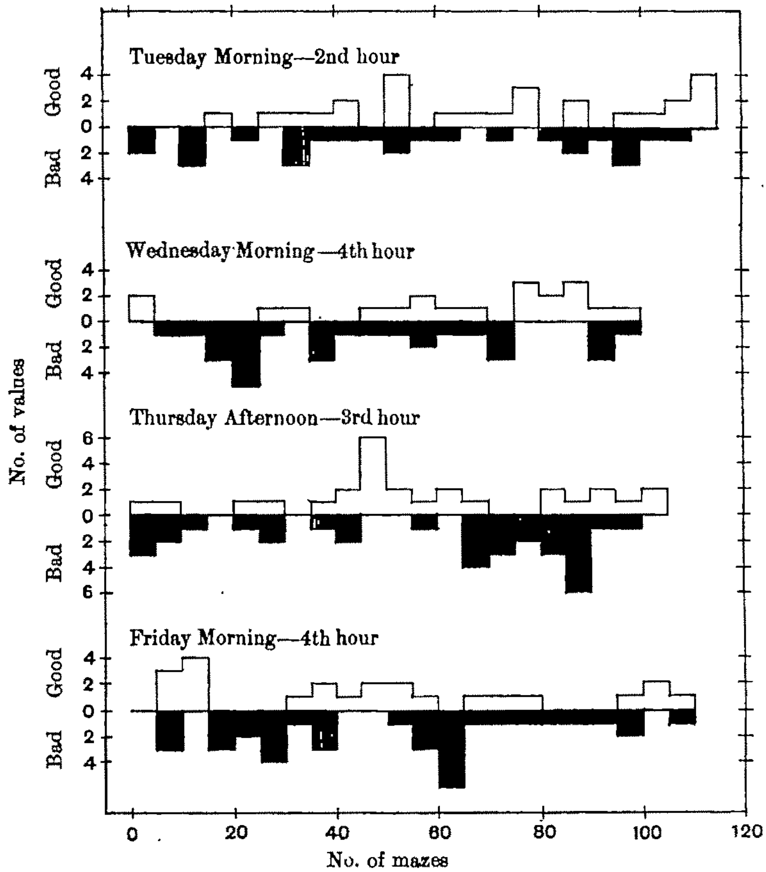


Fig. 2. Alternating periods of good and bad performances, Maze-tracing Test.

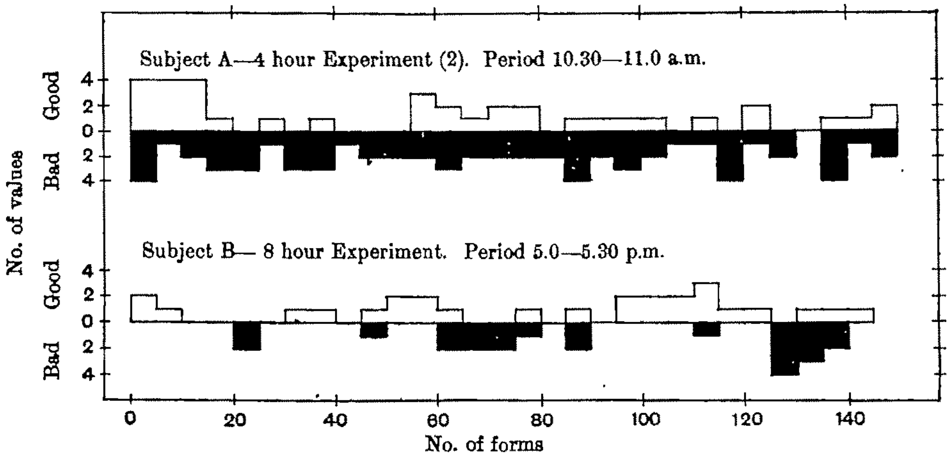


Fig. 3. Alternating periods of good and bad performances, Paper-folding Test.

attention to the work in hand; that is to say, it was almost completely automatic. The subject was able to carry on day-dreaming, and even thinking of a more rational type. Thus there was at no time any conscious speeding-up or slacking off, or attempt at increased accuracy—with the possible exception of Saturday morning; and the objective results obtained appeared to her to be generally uninfluenced by voluntary effort.

As long as thinking or day-dreaming of a satisfactory nature could be carried on there was very little boredom. But at certain periods, when the subject was obliged to attend, in whole or in part, to the course of the experiment, boredom of a more or less severe nature intervened. This became more frequent towards the end of the week in the Maze-tracing Test; and may perhaps indicate that some species of mental fatigue had set in. But such experiences are by no means unique in the subject's experience, and cannot with any degree of certainty be attributed wholly or even in part to the experimental work done.

These periods of boredom were usually just the periods of variability of performance described above. Typical examples are the fourth hour of the morning spell on Wednesday and Friday, and the third hour of the afternoon on Thursday; Friday afternoon also showed similar phenomena. Saturday morning, curiously enough, does not show great variability of performance, although both instrumental trouble and great boredom occurred to disarrange the performance—possibly as the result of the end-spurt mentioned above.

Certain periods of considerable sleepiness were also felt in the afternoons. The unsteadiness which would naturally be expected in such circumstances was very similar to that shown in periods of boredom, and may indicate that the subject was less independent of voluntary attention than she supposed.

Similar effects were observed by Subject *B* in the Paper-folding Test. But with Subject *A* the experience of boredom was much more severe. He found it impossible to work without attending in some measure to his performance; perhaps in consequence he experienced boredom or annoyance more frequently and more severely than Subject *B*. When this became acute he would make a spurt, which made his performance considerably more inaccurate and also frequently led to stumbling and folding the forms in the wrong order, thus breaking the rhythm of the performance. His variability was larger than Subject *B*'s throughout; but during such a spurt it was particularly marked.

Subject *B*, on the other hand, felt the unpleasant effects of this type of work more severely after the work was over. Though little conscious effort was felt at the time, it seems probable that such an effort was made to avoid thinking too much about the work and the boredom accruing from it. When the work was over, this effort was relaxed, with unpleasant after-effects of weariness and lassitude.

X.

It must be obvious that very little resemblance can be observed between the results of the various types of repetitive work described above. Muscio shows a gradual increase of accuracy up to the end of the third hour of work, both in his Aiming and Pendulum Tests; Teruoka a gradual increase up to about the middle of the spell, followed by a gradual decrease; the writer, in the Pendulum Test, a gradual decrease after the first hour of work. With the Paper-folding and Maze-tracing Tests no general tendency is discernible; except that in the latter accuracy is less in the afternoon than in the morning, a result more or less in agreement with Teruoka's Aiming Test and with the writer's Pendulum Test. This want of general agreement is hardly surprising on considering that:

(1) The nature of the tests is very different, the Aiming and Pendulum Tests involving much more muscular fatigue than the Paper-folding and Maze-tracing tests.

(2) Each test was tried on one or two subjects only and as a rule for a short period of time, so that variations and effects due to extraneous causes may have played a large part in the results.

(3) The measurements and objects of the experiments were different in different tests; that is to say, the Aiming and Pendulum Tests were devised to give measures of the general level of accuracy, but not a measure of output or rate of work or of variability—neither are any introspective data given.

It appears that more agreement may be expected between the results of one worker with different tests—provided they are of the same type—than between the results of different workers on the same test. Muscio shows a general agreement between the results of his Aiming and Pendulum Tests, but no agreement with Teruoka's Aiming Test or the writer's Pendulum Test. There is not much agreement between the two subjects in the Paper-folding Test; but the results of this test, for Subject *B*, agree fairly well with those of the Maze-tracing Test. The Pendulum Test is sufficiently different in operations and method from the Paper-folding and Maze-tracing Tests to make easily understood the want of agreement between the results; the muscular fatigue of the Pendulum Test is probably largely responsible for the continuous decrease of accuracy.

Thus the disagreement of the results obtained by different investigators on various types of work of a repetitive nature is more remarkable than their agreement. This manifestation of the importance of individual differences in such types of work will now be discussed.

XI.

It is urged that laboratory experiments on industrial fatigue and efficiency can have little bearing upon work actually carried out under industrial conditions[1]. Certainly the *a priori* arguments are in favour of this view. The difference in surroundings, method of work, duration of work and experience, object of work and mentality of worker, are immense. Are they, however, sufficient to militate against the usefulness of laboratory work such as has been described in this paper?

The laboratory possesses one undoubted advantage over the workshop or factory; in the former a study of individual differences is very much easier, both in objective performance and in subjective experience, together with the conditions under which they manifest themselves. The statistical methods generally necessary in the preparation of results obtained under industrial conditions usually reduce to insignificance any such individual differences. If the work of one individual is averaged over a period of one week, an output curve is produced which does not differ materially from many similar industrial curves[2]. The writer's weekly curve for the Maze-tracing Test resembles in form such curves, and shows a variation from the mean which is little, if any, greater. That is to say, whether an individual's performance is stable or variable, the probable result produced by averaging over a period of time under approximately unchanged conditions will be a stable one. How much more so, then, when a large number of individuals are averaged together! Industry is concerned with human beings in the mass, and in the mass will their performances be measured and the results of these performances interpreted. The importance of such results needs no enhancement; but there may perhaps be room for others.

The significance of these individual differences is the more considerable in that they probably indicate the presence of deeply-rooted underlying tendencies of a temperamental order, and not of some mere effect of chance. Thus, differences have been observed between the performance of the three subjects who undertook the Aiming and Pendulum Tests. These probably indicate a difference in the power of resistance to the peculiarly unpleasant mixture of muscular fatigue and mental irritation to which these tests were liable to give rise. This contention cannot be proved, since Muscio and Teruoka give no introspective evidence. But to the writer this test was much more unpleasant than the two tests subsequently performed. Hence it may have been more trying for her both physically and mentally—a fact which would explain the continuous decrease of accuracy observed in her results. Muscio, having more resistance, increased in accuracy for three hours; Teruoka's performance fell between the two.

---

[1] Cf. H. M. Vernon, this *Journal*, vol. xv, pt. 4.

[2] H. M. Vernon and T. Bedford. Report of the Industrial Fatigue Research Board, No. 25, p. 15.

Again, turning to a comparison of the performances of *A* and *B* in the Paper-folding Test we obtain more evidence of important differences. The great variability of performance of individuals similar to Subject *A*, and the frequency and severity of their feelings of boredom and annoyance[1] would make them unsuitable for employment on repetitive work demanding a moderate but steady level of performance over long periods of time[2]. Subject *B*, though less variable and less given to such feelings, suffered unpleasant effects from this kind of work after the work was over. Such a type of worker might not be unsuitable for relatively short spells of repetitive work; but would be overcome by longer periods. But no doubt there exist individuals who would be much more suitable for employment on repetitive work than either of these two subjects. The power of doing long spells of repetitive work at a steady level of performance without undue fatigue or boredom probably lies deeper than the habits acquired by education and upbringing. In the writer's opinion it is frequently connected with the ease with which a worker can detach his attention from the work in hand. It is not impossible that certain workers can apply their full attention to a repetitive task and obtain interest from it though it remains unvarying for days, months, and years. But those who are able to perform their simple repetitive task while in a state of day-dream or fantasy are probably far more numerous. To them repetitive and routine work is not boring and irritating; they may even prefer it to work which requires considerable application.

[1] These two experiences should be carefully discriminated, though their immediate effects upon the performance may be very similar.

[2] A comparison of the performances of *A* and *B* in a number of different tests supports this contention strongly.

Lastly, attention must again be drawn to the fact that Subject *A* was liable to make very large mistakes, and also would on some occasions break the rhythm of the task altogether. Such slips under industrial conditions might be responsible for an accident. When working with sharp tools or dangerous machinery motions of the hand made inaccurately or in the wrong order might easily result in cuts or injuries. Much work is being done at present upon industrial accidents, and those individuals who are by nature particularly liable to them. It is suggested that a worker who is unable to perform a repetitive task in a semi-automatic manner—that is to say, with only the necessary minimum of attention; whose performance is very variable, and who in moments of irritation and boredom, which are not infrequent, may be subject to considerable mistakes and inaccuracy—is also a worker who is liable to incur accidents of a more or less severe nature. Thus, even should it be difficult to find some rapid test for detecting persons prone to accident, it might be possible to discover them by repetitive tests of the order described in this paper.

XII. SUMMARY.

(1) The object of the work done has been to demonstrate the changes of output and accuracy of performance which occur in long spells of simple repetitive work which is not muscularly fatiguing. To this end have been employed the Aiming and Pendulum Tests, and the Paper-folding and Maze-tracing Tests.

(2) The work of different subjects with these different tests shows little agreement as to the general tendencies of output and accuracy of performance. Different observers have obtained:

(*a*) In the Aiming Test, a gradual increase of accuracy.

(*b*) In the Aiming Test, a gradual increase of accuracy, followed by a gradual decrease.

(*c*) In the Pendulum Test, a gradual increase of accuracy.

(*d*) In the Pendulum Test, a gradual decrease of accuracy following on a very short period of increase.

(*e*) In the Paper-folding Test, accuracy remaining fairly level throughout. A very slight increase of output.

(*f*) In the Maze-tracing Test, accuracy fairly level in the mornings, and less, though increasing, in the afternoons. Output level, but less in the afternoon than the morning.

Thus it appears that no general deductions can be made from the general trend of output and accuracy in the work of a few observers under experimental conditions. There seems, however, to be more correspondence between the work of the same observer on different tests, than between the work of different observers on the same test.

(3) In the Paper-folding and Maze-tracing Tests, both subjects showed a tendency to alternating periods of accuracy and inaccuracy.

(4) There appeared to be a frequent synchronization of these inaccurate periods with periods of feelings of intense boredom on the part of the subject.

(5) There was a considerable difference in the variability of performance of Subjects *A* and *B*, and in the number and magnitude of the inaccurate periods. There was a corresponding difference in the frequency and severity of the feelings of boredom, the subject who was the most bored being the most variable and the most liable to large errors.

(6) As the result of the study of individual differences, which is carried on more easily under the regulated conditions of the laboratory than in the industrial works, it appears probable that certain persons are temperamentally unsuitable for repetitive work because of:

(*a*) Their variability of performance.
(*b*) Their liability to extreme boredom and discontent.
(*c*) Their probable accident liability.

(7) It appears to the writer that such individuals may be characterized by their inability to abstract their minds from the work in hand, to engage in other thought, and to perform their task more or less automatically.

These conclusions cannot in any way be considered as final. There is not sufficient evidence at present to prove them. With the employment of more subjects and more tests, however, it is hoped that they would be substantiated.

In conclusion, I wish to thank Mr Bartlett for his great help and kindness in planning, directing, and acting as subject in this work.

(*Manuscript received* 28 *October*, 1924.)

THE DISCRIMINATION OF WOOL FABRICS BY THE SENSE OF TOUCH[1]

BY HENRY BINNS.

I. *The Psychological Aspect of the Wool Trade* (pp. 237, 238).
II. *The Experiments, Subjects and Materials* (pp. 238, 239).
III. *The Suggested Conclusions* (pp. 239–242).
IV. *The Difficulty and Significance of the Tests* (pp. 242, 243).
V. *The Relationship of Visual and Tactile Judgments* (pp. 243–245).
VI. *The Measurement of the Sense of Touch* (pp. 245, 246).
VII. *All-round Development of Ability* (p. 247).

I. *The Psychological Aspect of the Wool Trade.*

THE manufacturers of wool fabrics endeavour to supply the needs of the consuming public through the wholesale and retail distributing houses. Fashion is determined mainly at society functions, race meetings and on the stage. The Model Houses of London and Paris, where garments and cloths are shown under ideal conditions, also create a 'prestige' which influences the trend of sales to a remarkable degree. In addition, gifted persons with a fund of original ideas develop new styles and to them we are indebted for the displays seen in the shop windows.

The selling of ready-to-wear garments obviously involves psychological factors, but the selection and treatment of the materials are too often regarded as purely technical operations. A moment's thought will show the only method by which a woman is able to form a judgment of plain cloth is by the intelligent use of the senses of sight and touch. The producer in each of a great variety of processes has the same capacity for judgment, in addition to his technical skill. The ability to discriminate between finely graded cloths is assumed to be dependent upon a lengthy practical and technical training, but there is no evidence available showing how far this view is tenable.

The dress goods manufacturer does not supply material direct to the consumer, but through the distributing houses, both wholesale and retail, the buyers of which are not always technically trained, although they may have developed a fine 'mind-sense judgment' by responsible and extensive experience. These buyers not only judge by money value but also by style, colour, design and 'handle.' The educationist, the artist, the organizer or the journalist, is influenced mainly by the colour and design of fabrics—by the appeal to the eye. But the practical buyer of wool fabrics seems unable, when looking at materials, to keep his hands away from them. He thinks and speaks in terms such as: pliable, supple, soft, kind, lofty, firm, hard, or harsh—and all these are associated with the sense of touch. It seems probable that the stress laid on 'handle' results from a close appreciation of the requirements of the purchasing public, women in particular being attracted by the softness of wool goods.

---

[1] This paper was read to the psychological section of the British Association for the Advancement of Science, Southampton, 1925.

The chief difference between wool and other textile fibre lies in its elasticity, which has been found by experiment to be very highly correlated with softness as determined by 'handle'; the greater the elasticity, the greater will usually be the softness. Broadly also it may be said that fineness, shortness, whiteness, softness and elasticity go together, the finest wools being the most expensive and the coarsest costing the least.

II. *The Experiments, Subjects and Materials.*

With the object of defining the relationship of the senses of sight and touch an investigation was carried out by the writer, as experimenter, with the assistance of 35 dress-goods experts; 20 wool-men engaged in the early processes of manufacture, but untrained in the judgment of the finished fabric; 20 men and 50 women, mainly teachers without any trade experience, but with good intellectual attainments.

The materials used were manufactured at the Bradford Technical College, in co-operation with a group of experienced men from the Bradford Textile Society[1]. Three different lots of wool[2], *A*, *B* and *C*, were put through the same processes, alone or blended, and these finally provided three ranges of five cloths, making 15 cloths in all. Five of these were left in the undyed state; five were dyed alike and given a

---

[1] The Chairman of the Investigations Committee, Mr A. M. Chapman, manufacturer; Prof. Eber Midgley, Head of the Textile Dept., Bradford Technical College; Mr Jos. Dumville, Spinning Lecturer; Mr S. B. Hollings, a well-known wool expert; Dr Wynn-Jones, Psychologist of Leeds University, have read this paper and agree with the conclusions.

[2] The origin of the wools was as follows:

A. 64's South Australian Merino (Hope Para).
B. 64's Central Queensland Merino (Malvern Hills).
C. 58's Victoria Comeback (J. F. Rubicon).

AB, *AC*, *BC* were 50 per cent. blends of the respective qualities.

'clear[1]' finish and the remaining five were also dyed alike and given a 'milled' finish. Theoretically, each group of five cloths should be graded in precisely the same order.

Lots *A* and *B* wools each had an average diameter by micrometric measurement of 1/1250″: a 50 per cent. blend of *A* and *B* would therefore be the same, *i.e.* 1/1250″. Lot *C* measured 1/950″, but this was only used in blend with *A* and *B*; so that a 50 per cent. blend of *A* and *C* contained one-half 1/1250″ and one-half 1/950″, whilst *B* and *C* would be exactly the same, viz. 1/1250″ and 1/950″. Again, theoretically, the cloths made from *A*, *B* and *AB* should be alike and each of them should be better than the lower quality blends of *AC* and *BC*.

The subjects were asked to grade each range of cloths, first by sight alone, for 'smartness of appearance,' the experimenter placing the cloths in rank order, as desired by the subject. Then, after careful shuffling, the cloths had to be graded by touch alone for 'softness of handle,' great care being taken to avoid even a casual glance at the cloths. Blindfolding was thought to be unsuitable, being unnatural and likely to disturb the normal judgment: experience has shown this view to be sound. Lastly, the cloths were graded on a commercial basis of unfettered judgment, the instructions being to "place the cloths in the order in which you would buy them if all were offered at the same price"—any deliberate or subconscious preference might thus be biassed towards smartness of appearance, softness of handle, or other characteristics, according to the fancy of each person.

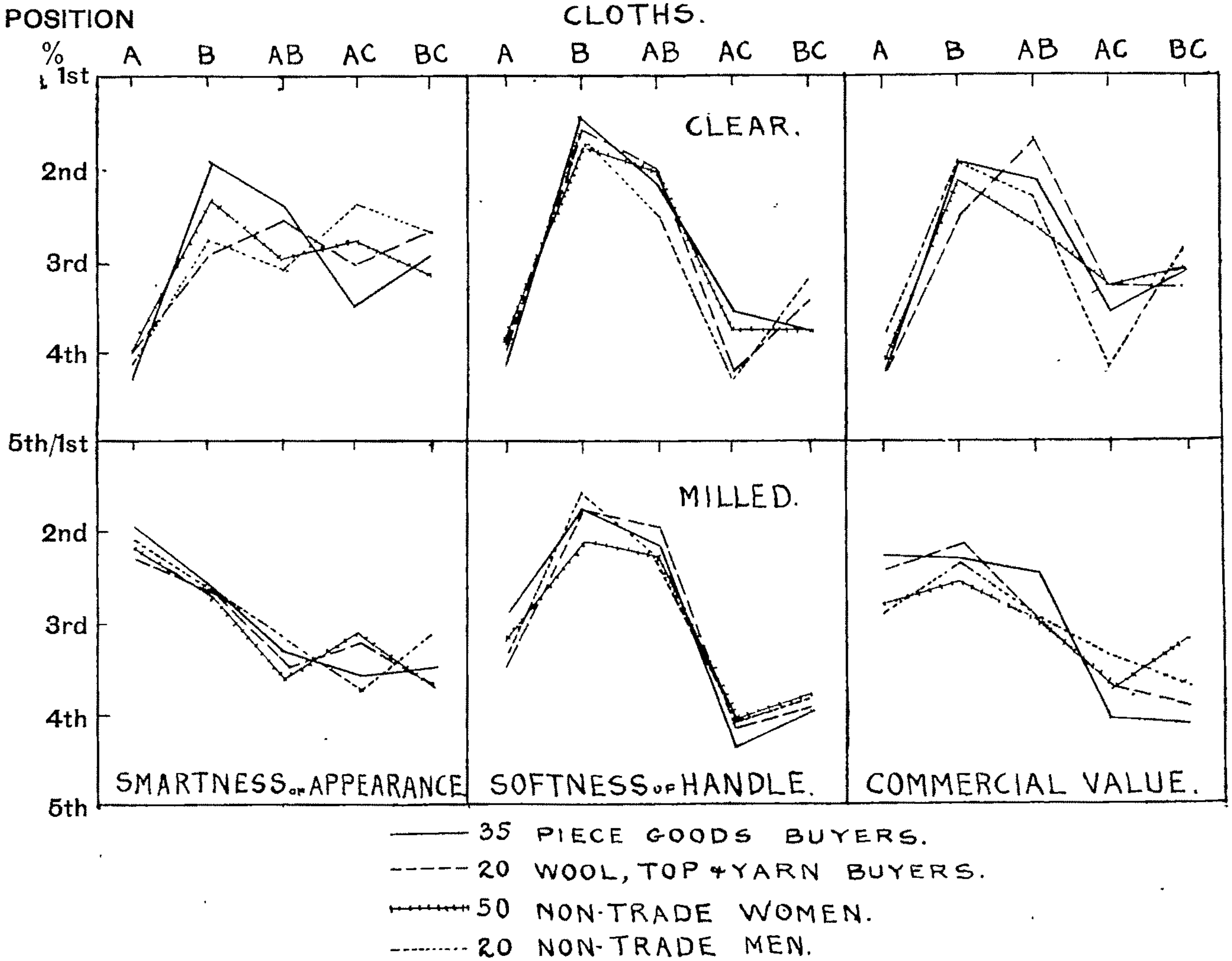
The commercial aspect of the enquiry concerns the developments of the latent properties of the three wools during manufacture; whether the changes affect the appearance, handle, or value; and how experienced men in the raw and semi-manufactured materials sections, and piece-goods experts, differ in their opinions from untrained intelligent men and women who are the prospective customers.

III. *The Suggested Conclusions.*

The details of the investigation may be more readily grasped if the main conclusions are suggested. Graph 1 is based on a percentage cost and affords a useful means of comparison.

---

[1] *'Clear' finish* indicates an absolutely clear surface, free from all loose and straggling fibres. By reason of this it has a thready appearance in which the design and twill are clear. A 'clear' finish feels thinner than a 'milled' finish. *'Milled' finish* has a full handle. There has been more fibre movement, the curling up of the fibres being responsible for the development of a fibrous surface. The appearance of the threads and with it the character of the design and twill is diminished.



Graph 1. The wools used in cloths *A*, *B* and *AB* are of equal fineness measured by micrometer. Wools *AC* and *BC* are also alike in diameter.

1. There is a striking uniformity between the average judgments of the four groups of trained and untrained individuals on smartness of appearance, softness of handle and commercial value.

2. The uniformity is most marked in the grading of the touch judgments, the intercorrelation by Pearson's Product Moments Formula being in the case of the clear finish + ·948 ± ·030 and the milled finish + ·972 ± ·016. It is safe to say that no experienced manufacturer or buyer of these fabrics would ever have imagined such results possible. The blending of a lower priced wool with the better qualities is definitely detectable by those judging whether in the trade or not.

3. By sight alone the agreement is + ·764 ± ·123 on the clear finish, showing that the opinions are not quite as decided as on touch. There is, however, a slightly higher correlation of + ·851 ± ·079 in the smartness of appearance of the milled cloths. This suggests that whilst the introduction of a lower quality of wool is not as readily detectable on the ground of appearance as on handle there is, nevertheless, a strong agreement between the skilled and unskilled groups of judges.

4. It seems remarkable that on purely commercial judgments the opinions of manufacturers and ultimate purchasers should be in almost complete agreement, the correlations being + ·855 ± ·078 on the 'clear' finish and + ·876 ± ·073 on the 'milled' finish.

5. The tendency in both trained and untrained judgment is to combine sight and touch in the estimation of commercial value, the bias to sight or to touch varying according to the construction and finish of the cloths.

6. Both ranges of cloths were dyed from the same grey (undyed) cloth. The average correlations of the 35 experts with those of eight experts judging the undyed cloth are for

	Finish	
	Clear	Milled
Smartness of appearance	+ ·473 ± ·234	+ ·610 ± ·112
Softness of handle	+ ·710 ± ·149	+ ·977 ± ·013
Commercial value	+ ·558 ± ·208	+ ·905 ± ·054

The inference is that the most important difference between the cloths is in the material of which they are composed. Where the finishing treatment has caused more fibre movement, as in the case of the milled finish, the softness is most developed to the detriment of smartness of appearance. The characteristics due to handle remain dominant in the assessment of commercial value.

7. Fibres of equal diameter are seen to have changed their characteristics during manufacture and thus developed either a smarter appearance or a softer handle.

IV. *The Difficulty and Significance of the Tests.*

Most persons attempting the tests found very little difference between the cloths. One important wholesale buyer said he "should have thought they had all been cut from one piece," whilst another said he had "seen plenty of goods with these variations delivered in one lot." A technically-trained man with a long experience said: "For an ordinary buyer it is a knotty point dealing with cloths so near alike as these are; it is very much like dipping into a lottery and the knottiest point of all is by full judgment" (commercial value). On the other hand, a retail buyer declared that "Women have a far better and keener sense of quality than the average man; it is uncanny the way women get at it."

The difficulty of the tests cannot be ignored in comparing the abilities of skilled men and intelligent women, nor can their striking resemblance be explained other than by the common factor of natural ability, as distinct from technical ability.

In previous experiments it has almost invariably been found that in estimating the ability of individuals or groups, two factors must be considered. The first might be termed 'Decision'[1]—being the uniformity of repeated judgments, and the second a 'Trade' opinion, being the relative correlation of a person or a group of persons with a trade criterion arrived at by averaging the judgments of a number of experts. Whilst the trained[2] and untrained opinions may on average reflect approximately the same trade judgment, the trained individual or group is more pronounced and decided than the untrained, which might reasonably be expected. This is shown in the average correlation of each individual in the two main groups with the trade criterion.

---

[1] The Germans use 'Reliability of Judgment' (*i.e. Zuverlässigkeit der Urteile*) obtained from the mean variation as in 'Decision'; 'Sensitivity' (*Empfindlichkeit*) is used to indicate trade opinion. The meaning in both cases is the same but the decisive 'Yes' or 'No' opinion of the business man on a trade judgment may be more readily understood in commercial circles than the more precise interpretation of psychologists.

[2] The exact meaning of the word 'trained' is difficult to define as each individual is being unconsciously trained in his everyday experience. Certain types of hand-training in schools can be shown to have a good or bad influence on fine discrimination of wool. Trade judgments demand concentration of mind through the senses of sight and touch and may in consequence be classed as specifically trained.

	Smartness of appearance	Softness of handle	Commercial value
55 Wool textile men	·36	·69	·55
70 Non-trade men and women	·26	·59	·40

It might roughly be stated, therefore, that training would be expected to raise the general level of judgment on all three counts by 20 per cent., this being approximately the degree of 'decision' imparted to a natural judgment by training.

An eminent wool buyer said, on viewing the samples of wools *A*, *B* and *C*, in grease, "Anyone who knows anything about wool would grade these marks in the following order whether straight or blended: it is a well-known fact that *B* comes up fine and *A* comes up harder—*B*, *AB*, *A*, *BC*, *AC*."

The final grading of 35 experienced wholesale and retail buyers of this type of material is in precisely the same order as that of wool buyers knowing the origin and mark of the wool, viz.:

Rank	1st	2nd	3rd	4th	5th
Wool buyers' opinion	*B*	*AB*	*A*	*BC*	*AC*
35 Piece buyers' opinion	*B*	*AB*	*A*	*BC*	*AC*

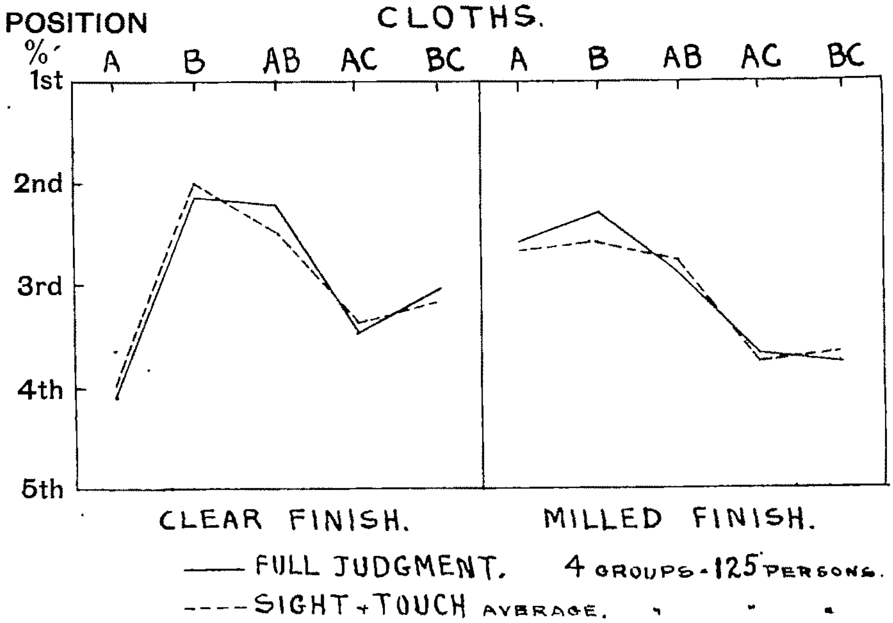
This shows that knowledge of the characteristics of particular wools may be exceedingly valuable in estimating the properties of cloths in the finished state, ready for sale. The experiments show also the possibility of proving that which otherwise may be merely a pious opinion. It seems obvious that what appears to be casual has a more sound basis than was supposed.

V. *The Relationship of Visual and Tactile Judgments.*

It will be noted that in both the clear and milled finishes the judgments on commercial value appear to combine the independent judgments made by sight and touch. Graph 2 shows in a striking manner the decisive correlation existing between an average of the two senses and by unfettered judgment in which both senses are acting jointly.

Calculated on the Product Moments Formula the correlation visualised in Graph 2 is stated in figures in Table II as follows:

	Finishes	
	Clear	Milled
The average of Sight and Touch with Full Judgments	·985 ± ·008	·983 ± ·010
Sight alone with Touch alone Judgments	·659 ± ·169	·280 ± ·276
Full Judgments on Clear and Milled finishes ...	·307 ± ·272	



Graph 2. Shows the average of the four curves in Graph 1 for 'smartness of appearance' (sight), and 'softness of handle' (touch) compared with the same for 'commercial value' (full judgment). The correlation between these factors is ·985 in clear finish and ·983 in milled finish, the 'probable error' being covered 100 times, three times only being necessary to cover the operation of pure chance. Stated commercially, the differences are ¼*d.* and ¾*d.* a yard respectively on a basis of 8*s.* per yard.

The real significance of these figures lies in the perfect agreement existing between judgments in which all faculties are allowed full scope and the sum of the judgments by sight and touch acting in complete independence. This is remarkable because the senses of sight and touch are only in moderate agreement on the clear finish, and seem to have worked quite independently on the milled finish cloths. Further, though the two ranges were dyed from the same grey cloths, they are shown, in the opinion of 125 persons, to have no common factor in grading when dyed. The active co-operation of the senses in wool-textile judgments is thus clearly demonstrated.

Small samples (6″ by 6″) of the clear finish cloths were submitted through the post by a merchant to several manufacturers, in order that absolutely impartial judgments of commercial costings might be secured. The correlation between the average of these costing and the average of the full judgments of 125 persons previously alluded to is + ·768 ± ·122. These figures indicate that these manufacturers were able with considerable accuracy, even from small samples, to analyse and judge minute differences between cloths.

Mechanical tests were made in the laboratory of the Bradford Technical College for the strength, soundness and elasticity of the same samples. The average of these tests correlated with the full judgments of the 125 persons at + ·378 ± ·256, the probable error not being covered twice.

It would appear, therefore, that valuable as the well-recognized mechanical tests undoubtedly are, they do not necessarily reflect the selling value of cloths. Psychological tests should in consequence be regarded as an additional factor in the scientific measurement of minute differences in wool fabric construction.

VI. *The Measurement of the Sense of Touch.*

Many attempts have been made by investigators to measure the softness of fabrics by mechanical appliances, and by other methods, but so far as I am aware all have been abandoned. The alternative is to find the most delicately adjusted human measuring machine and to form methods by which persons possessing exceptional abilities may be used in experimental work.

The uniformity of the tactile judgments in these tests suggested the possibility of measuring the sense of touch in its commercial aspect, and in its most highly trained form. The average of a number of experts appeared to give a sound trade criterion with which could be compared the judgments of boys and girls wishing to enter the wool trade. An obvious advantage is indicated by the fact that these particular tests, in which wool blending was the chief feature, did not favour those individuals who had been specially trained on wool, except in so far as it was possible to register a greater degree of discrimination by touch due to practice.

By way of illustration four cases are selected to represent the types of discrimination met with in a district almost entirely engaged in the wool trade, the comparison being on 'Decision' and agreement with 'Trade' opinion:

	Decision	Trade
Subject *A*	+ ·99	+ ·96

Might be said to have been cradled in wool, his father being a well-known practical man and lecturer, and also an enthusiastic believer in early training. The son, aged about 30 years, did excellently at school and also at a Technical College. He passed every examination at school and the highest honours in technology without a miss. He has had an unusually wide experience in buying, selling and manufacturing, and held a most responsible position at a high salary in his early twenties. Other tests on different classes of wool materials gave similar results. This man seems to be an outstanding judge.

	Decision	Trade
Subject *B*	+ ·82	+ ·96

Was about 19 years of age at the time of testing. A tall, delicate lad, growing too quickly, missed school periodically but was always regarded as sharp. The Headmaster of a Grammar School considered him worth training for a Barrister, but he and his parents decided on the wool trade, and he was placed with a first class firm of wool merchants, to learn wool-sorting, with the object of advancing to a buyer's position. The results indicate an extraordinary appreciation of fine differences by touch with a sound trade bias. The boy had only been handling wool a month or two prior to testing and neither of his parents had been connected with the wool trade; both are refined people and have artistic tastes. The expectation would be a rise in Decision from ·82 to ·95 or over by a long period of training. This seems to be a case of outstanding natural touch ability which would obviously be a gain to the wool trade and which would be of no particular advantage in the legal profession.

	Decision	Trade
Subject *C*	+ ·58	+ ·86

Was 16 years of age, had just matriculated and was attending a Technical College to study wool. He was described by the Headmaster as a "heavy type both in mind and body." Yet it will be seen that his agreement with the trade shows a most desirable trait, well worthy of training. Though his Decision is relatively low the detailed figures show signs of ability capable of development. On one of the ranges of cloth found to be most easy to grade he averaged ·97 over the ten intercorrelations: on the next ·72 and on the most complex ·07. This is probably a typical case in which a good natural ability may be greatly improved by appropriate technical training, but which, if left to trial and error methods, might result in lowering his agreement with the Trade, without raising the Decision.

	Decision	Trade
Subject *D*	+ ·34	+ ·69

Was also 16 years of age and in the same form as *C*, matriculating at the same time. The Headmaster described him as "very good at languages and mathematics." "He is almost an effeminate boy, interested in languages and music but without any practical ability. I cannot imagine him going into business; he is cut out for figuring on the thinking side of accountancy. He is an academic rather than a practical boy." On the easiest range he scored ·91, but only ·06 and ·07 on the more difficult ones. His final ranking of the cloths was wide of the trade criterion. This boy might be very happy in his contemplated sphere of accountancy, but would most probably be worried and cause considerable trouble to an employer in the wool trade, without either of them realising the underlying causes of their difficulty.

	Decision	Trade
Subject *E*	− ·06	+ ·27

Elementary school boy (14 years of age) very much below the average at school; has no real discrimination and it would be impossible to train him for any position in which touch is required.

VII. *All-round Development of Ability.*

In conclusion, the general impression gained by such experiments has been that suitable training is essential even to the most gifted persons, in order to produce a first judgment which shall be equal to the average of a number of judgments by the same person[1]. A Headmaster's opinion is usually sound concerning boys, but there is no standard by which comparison may be made: tests similar to the ones described supply the need within limits. If the principle is accepted, any degree of differences of handle may be made by the blending of wools. The samples used in this enquiry are, however, so finely graded as to make it practically impossible for any person to grade them perfectly five times.

Tests of this character tend to emphasize the importance practical men place on personal judgment and emphasize the need that more attention be given to the whole subject of mind-sense judgments by men of science.

The tests also show the importance of innate ability and its connection with general and technical training. The need for all-round development of ability is stressed. One faculty should not outweigh another in unfettered judgment. The standard of perfection in wool textiles should be improved if the basic principles on which judgment rests are analysed and tabulated.

Such tests also point to the need for attention being given to the scientific aspect of buying and selling as distinct from the productive side of the wool trade.

---

[1] Such training could readily be given by providing suitable samples and by systematic practice.

OBITUARY

Dr HENRY J. WATT.

1879–1925.

Psychology and music have suffered a serious loss in the sudden death of Dr Henry J. Watt, Lecturer on Psychology in the University of Glasgow. In the summer of 1914 he paid a visit to his old friend and teacher, the late Prof. Külpe, and had the misfortune to be interned in Germany for about twelve months. The rigours of war-time fare told on his somewhat delicate constitution to such an extent that he was permitted to return home in the summer of 1915, but in a state of decrepitude from which he had not quite recovered when on the 25th of October, 1925, at the early age of 46, he succumbed to a heart attack.

As a student of philosophy at the University of Aberdeen he gained a reputation for ability and originality of thought which became European when he published as a doctorate thesis at Würzburg his *Experimentelle Beiträge zu einer Theorie des Denkens*. On his return to England in 1907 to take up duty as Lecturer on Psychology in the University of Liverpool and next year in the University of Glasgow his psychological interests passed from the thought-processes to the sensory processes, and he began a line of thought which he pursued with great vigour to the day of his death. It found expression in numerous papers in this *Journal*, in his *Psychology of sound*, his *Foundations of music*, and his last book, *The sensory basis and structure of knowledge.*

Dr Watt's early philosophical training showed itself in a love of systematizing, while his training in the laboratories of Würzburg and Berlin gave him the confidence in the methods of experimental psychology which characterizes all his writings. He strove vigorously to work out a 'pure' psychology, and never tired of distinguishing the problems of physics, physiology and psychology: his theory of sound is a determined effort to state a purely psychological theory.

Thanks mainly to his accomplished wife, who was a pianist of the first rank and from the shock of whose death three years ago he never quite recovered, he lived in an atmosphere of music; hence, probably, his keen interest in the psychology of sound and music. As a thinker his originality and honesty of purpose commanded the admiration of all who knew him: as a friend and teacher he was always kind, genial and inspiring.

S. D.

CRITICAL NOTICE

BY E. O. LEWIS.

The Young Delinquent. By CYRIL BURT, M.A., D.Sc. Pp. vii + 635. University of London Press. 1925. 17*s*. 6*d*.

This is the first of three books which Prof. Burt proposes to write on sub-normal children. To say that he has written a book of interest to a wide circle of readers, would have an unfair implication. Dr Burt has studied his material so thoroughly, and has viewed his problem from so many different standpoints, that his observations and results are inevitably important to scientists and social workers of diverse interests. This first volume deals with the delinquent; and it is limited to the study of the young delinquent, a point that should be borne in mind in any application of Dr Burt's conclusions to the practical problems of criminology. The method of approach is predominantly psychological; but the book will interest not only psychologists, but also educationists and teachers, doctors and prison officials, and parents and social workers. Dr Burt has presented his material in a most readable manner, and the book has no small claim to literary merit. His pen pictures of several of these young delinquents are truly fascinating. There is much subtle humour and repressed pathos, which Dr Burt has tempered, no doubt unconsciously, so as not to disturb unduly any conservative susceptibilities which the reader may have.

The work is based upon the individual study of 200 delinquent children. A most valuable feature is the parallel study of a control group of 400 non-delinquent children, of similar ages, the same social class, usually living in the same streets, and attending the same schools as the delinquents. It is natural to compare and contrast Dr Burt's results with those of Dr Healy in his volume on *The Individual Delinquent.* The general conclusions of these two investigators agree fairly closely; but Dr Burt indicates several instances where his results differ from those of Dr Healy. It is very difficult to compare the data of the two investigations. One is the study of the adult recidivist whose mentality was so obviously abnormal that the magistrate thought it necessary to refer the case for medical examination; whereas the other is the study of the youthful first-offender. Healy studied delinquency in its maturity, whereas Burt has studied it in its infancy. With such different material it is not surprising that results show certain disparities. To cite but one instance. Dr Burt comes to the conclusion that insanity is a relatively unimportant causal factor of delinquency. In his group of delinquents there was only one case of *dementia praecox* (although 25 per cent. of his cases had reached the age of puberty); and only four cases were cited as psychopathic (but three of these were subsequently certified insane). Dr Healy, on the other hand, in his study of a thousand recidivists found 5·7 per cent. suffered from major psychoses. Dr Norwood East in his investigation at Brixton prison found that twice as many of the prisoners on remand were insane as were feeble-minded. Dr Burt no doubt rightly errs on the side of caution in placing young delinquents in the category of insane persons; but probably several of his delinquents will sooner or later deteriorate to a psychotic condition. Little is yet known of the manifestations of incipient insanity; but there is increasing evidence that many conditions which hitherto have been diagnosed as temperamental deficiency or adolescent instability are really conditions of incipient insanity. This is one explanation of the disappointing number of failures amongst the temperamental defectives which have been placed on probation in situations where they do not receive the supervision they had when at institutions.

Dr Burt finds the incidence of mental deficiency to be 8 per cent. in his group of delinquents. As compared with the figures of some American investigators, this estimate is low; but, on the other hand, some British investigators estimate the incidence still lower. The percentage of delinquents classed as "dull" in this investigation is as high as 25·7—a decided contrast to the percentage of "dull" in the control group, which was only 9·7. The respective percentages of those educationally backward in the two groups are 56·8 and 15·7. These data will naturally raise the question in the mind of the reader of the mental age limits of feeble-mindedness. Dr Burt maintains that adults with a mental age as low as eight may not be certifiable as feeble-minded; and in a previous work of his, he has given evidence which strongly supports this view. Enthusiasts, scarcely scientific enthusiasts, in the field of mental deficiency, will suspect the author of placing stress too exclusively upon responses to mental tests; and of not attaching sufficient importance to what is termed "conduct." It is true that Dr Burt has been, to use the analogy of the Bessemer process, one of the most active de-carbonisers of the too prevalent tendency in previous years to certify all troublesome cases as mentally defective. He attaches much importance to the responses of the individual to standardised mental tests, and relatively less to reports of conduct which he, as a clinical investigator, cannot observe directly. Dr Burt, however, has gone far to complete the Bessemer process in his chapter on "Temperamental Conditions"; and after reading this chapter, those who have charged him with adopting too intellectual a standpoint will probably recant. The general impression Dr Burt leaves with us is that he does not attach so much importance to mental deficiency as a causative factor of delinquency, as Dr Healy does; and that the psychologist has yet to find an explanation of recidivism.

Several chapters deal with abnormal instinctive and emotional behaviour of delinquents. The author attributes the disparity between the numbers of male and female criminals (a proportion of four to one) to the fact that the sthenic emotions are more pronounced in the male than the female. In 12 per cent. of the delinquents investigated by Dr Burt, an excessive strength of some particular instinct seems to be the sole or main cause of the children's offences.

Readers of this journal are already conversant with Dr Burt's conception of the "Temperamentally Defective." In this class he places persons who, without being intellectually defective, exhibit from birth or an early age permanent emotional instability, so pronounced that they require care, supervision and control for their own protection or the protection of others. About 9 per cent. of the delinquents were placed in this group; and if, as the definition suggests, the classes "intellectually defective" and "temperamentally defective" are mutually exclusive (although probably the majority of the temperamentally defective belong to the class "dull"), the total percentage for mentally defective persons in this group of delinquents would amount to seventeen. Dr Burt makes the interesting suggestion that the conception of temperamental deficiency should be substituted for that of moral imbecility. There is no doubt that most of the cases hitherto certified as moral imbeciles were merely temperamentally defective. It is instability rather than deficiency that is the hall-mark of the temperamentally defective described in this volume. Their instinctive behaviour and emotional responses are excessive rather than deficient. Their sub-normality is due to their failure to control and organise their instincts and emotions so as to preserve the balance expected of normal persons of their age. The question arises whether this description tallies with the descriptions of classical cases of that rare type of defective the moral imbecile. Is not absence of emotion rather than excess the characteristic feature of those moral imbeciles who have been notorious criminals? In these cases there seem to have been a functional derangement of the normal relationship of instinct and emotion. This, however, is a point of theoretical rather than of practical interest.

The emphasis placed in these notes upon the relationship between delinquency and mental deficiency is liable to give a wrong impression of the standpoint of the author of this volume. But one reason for discussing at length this portion of the work is that too frequently the magistrate or social worker assumes that the only service the psychologist can render is to decide whether the delinquent is mentally defective. Dr Burt gives many examples which prove that the psychologist can render much wider service in this field. In fact, Prof. Burt's standpoint seems to be that delinquency in the large majority of cases is not a pathological condition. In the concluding chapter he writes: "crime is assignable to no single universal source, nor yet to two or three; it springs from a wide variety, usually from a multiplicity of alternative and converging influences. On an average each delinquent child is the product of nine or ten subversive circumstances, one as a rule predominating and all conspiring to draw him into crime." The circumstances noted, however, are far from peculiar to delinquent families. The same circumstances were observed in the non-delinquent cases on an average, three per case as against nine or ten with the delinquent. Dr Burt has also ascertained the relative frequency of these circumstances as causative factors. The four most frequent causes were defective discipline, specific instincts, general emotional instability, and morbid emotional conditions. The four least frequent causes were physical infirmity of the child himself, poverty and its concomitants, family history of physical weakness, and family history of temperamental disorder such as insanity. Dr Burt comes to the conclusion that it is impossible to attribute crime in general either to a predominantly hereditary or to an environmental origin. Heredity appears to operate not through the direct transmission of a criminal disposition, but indirectly through such constitutional conditions as dull or defective intelligence, excitable and unbalanced temperament, or over-development of some single primitive instinct. Of environmental conditions, the home was found to be the most important; and the defective relationship between the parents and child was a factor of much greater potency than poverty. Dr Burt is very insistent that psychological factors are predominant in determining delinquency; emotional factors are relatively more important than the intellectual; and psycho-analytical complexes everywhere provide a ready mechanism for the direction of overpowering instincts and of compressed emotional energy into open acts of crime.

These are some of the many conclusions arrived at by Dr Burt that will disturb many of our preconceived ideas of the delinquent; and their practical implications are far-reaching. It is true the group studied was rather small; but the results obtained from a thorough study like this of individual cases, has greater scientific validity than vague statistical studies with somewhat superficial observations of the individual delinquent.

PUBLICATIONS RECENTLY RECEIVED

Helmholtz's Treatise on Physiological Optics. Translated from the third German Edition. Edited by JAMES P. C. SOUTHALL. Published by the Optical Society of America. Vol. I. 1924.

To arrange for the translation and publication of this monumental treatise of Helmholtz was a splendid enterprise on the part of the American Optical Society. As the editor points out it is now considerably more than a decade since the "publication of the posthumous third edition of the *Physiologische Optik* which was brought up to date and greatly enlarged under the collaboration of Nagel, Gullstrand and v. Kries. Yet in all these years there has been no English translation of this great classical treatise, and unfortunately no similar work in English of any kind." There is no need, at this date, to write anything about the extreme importance, for the student of the physiology and psychology of visual processes, of this great book. But it must be said that the translators, of whom there were naturally a considerable number, have performed their work with conspicuous success. They have, perfectly rightly, translated freely, so that an English version is produced which is both straightforward and pleasurable to read; but at the same time they have remained singularly faithful to the spirit and meaning of the original. To this volume is appended an interesting discussion of Ophthalmoscopy taken from Professor Gullstrand's book *Einführung in die Methoden der Dioptrik des Augen des Menschen.* The book is beautifully printed and produced. This and the succeeding volumes should help to stimulate a renewed study of the special senses in this country, for unquestionably and unfortunately England has fallen behind some other countries in this field of research in recent years.

This book is not offered for sale in the usual way, but it may be obtained from Professor F. K. Richtmyer, Secretary of the Optical Society, Cornell University, Ithaca, N.Y., U.S.A. The price is $7 a volume, or $21 for the complete set of three volumes.

The Essentials of Mental Measurement. By WILLIAM BROWN and GODFREY H. THOMSON. Third Edition. Cambridge University Press. 1925. The Cambridge Psychological Library. Pp. x + 224. 17*s.* 6*d.* net.

It is encouraging to find that a third edition of this very excellent book has already been called for. The alterations effected have not been very extensive, but are all directed to acquainting the reader with recent work on psychological applications of statistical methods and results. Thus a considerable number of footnotes and text references have been added to important recent work. An admirably lucid, if short, chapter has been written entitled *The Present Position* (1924). In this the issue as it is left at present between Spearman's '*g* theory' and Thomson's Sampling Theory of Abilities is very fairly stated. Professor Thomson adheres to his position as it was worked out in the 2nd edition of this work: "The controversy between us and Professor Spearman is not, and never was, as to the possibility of postulating a general factor, but as to the possibility of explaining all correlations thus without postulating any but the slightest group factors, and these very narrow in their action. Our position is that until the evidence is more clear we shall continue to suspect that numerous and wide group factors are present."

Experimental Investigations into the Emotional Life of the Child compared with that of the Adult. By HELGA ENG, translated by G. H. MORRISON. Oxford Medical Publications. 1925. Pp. vi + 243. 21*s.* net.

The German edition of this important monograph has already been reviewed in this *Journal* (XIV. 1923, pp. 107, 108). The English translation is a good one, and the book, with its interesting series of plates is beautifully produced. The volume not only gives a clear and stimulating record of valuable original research but in its course presents an admirable survey of practically all the important work in the field covered. It should encourage fresh work in this country upon a subject which is still full of unsolved problems.

Drei Abhandlungen zur Gestalttheorie. Von MAX WERTHEIMER. S. 184. Verlag der Philosophischen Akademie Erlangen. 1925. brosch. R.M. 6.—gbd. R.M. 7.80.

These three papers, which Professor Wertheimer has now published together in response to numerous requests, deal with very different subject matters, but are inspired by the same underlying notions. The first article is the extremely important "Experimentelle Studien über das Sehen von Bewegung." This occupies the major part of the volume. There follow a study of the development among primitive peoples of the notions of numbers and their relations, and a discussion of the logical process by which, given certain premises, we arrive at an apparently novel conclusion. The three papers are intended as an illustration of how the *Gestalttheorie* can successfully handle concrete problems belonging to different fields of study. Beyond question the first paper, with its array of ingenious experiments, is the most impressive. Wertheimer's excursions into comparative psychology and logic are here somewhat hurried, but as always his remarks are full of interest and forcefulness.

Beiträge zur Phänomenologie der Wahrnehmung. Von WILHELM SCHAPP. S. 125. Verlag der Philosophischen Akademie Erlangen. 1925. brosch. R.M. 3. —gbd. R.M. 4.50.

This brief but comprehensive treatise deals wholly with epistemological problems concerning the nature and validity of the objective reference present in all perceptual processes. The author began his work before the war, largely under the influence of Husserl, the school at Munich, and various other thinkers, particularly Conrad, Reinach and Neuhaus. The fighting and its consequences interrupted his task, which, however, he continued as far as possible, and now publishes. He begins by a discussion of the revelation of an external world in visual, auditory and other sensory experience, and proceeds to deal with the more complex perceptions of space. His preoccupation throughout is, not with the nature and conditions of the mental processes, but with the character and validity of the references beyond themselves which they appear to involve.

The Neuroses of the Nations: The Neuroses of Germany and France before the War. By C. E. PLAYNE. London: George Allen & Unwin, Ltd. 1925. Pp. 468. 16*s.* net.

Mrs Playne believes that the increasing strain of civilized life in the years immediately preceding the Great War produced in the main belligerent nations an unstable and diseased social state, which can be most aptly described in the language of psychiatrists. Her book is an attempt to apply this thesis to Germany and France. It is an undoubtedly interesting and provocative piece of work. But there is more assertion than proof, more use of the terms of psychiatry than genuine attempt to understand their precise significance. The book would have gained by condensation. When everything is said, however, it seems indisputable that it is only by attempts such as this, to deal directly with concrete social situations, that social psychology will make any advance worth noting. For that reason in particular this book deserves wide, if critical, attention.

The Mind in Health and Disease. By T. WADDELOW SMITH. London: Baillière, Tindall & Cox. 1925. Pp. viii + 235. 10*s.* 6*d.* net.

This book is intended as an introduction to a study of the normal and abnormal functioning of mental processes and is addressed to "students and general practitioners interested in mental work." Part I deals with the structure and functions of the nervous system, and to some extent with descriptive normal psychology. Part II treats of various disorders of function, and with the different forms of mental disease. In many ways the book is disconcertingly definite about matters that are gravely in dispute, and the whole treatment is exceedingly condensed. Definitions and classifications are everywhere; discussions play but a small part. But many of the diagrams presented are excellent, and as a book for swift consultation it may certainly be recommended. There is not a little to be said for the simplification of the complex problems of mental disorder, but it is much to be hoped that this book will be used mainly as its author clearly intends that it should be: as an introduction to more careful and critical studies. To this end it would have been of great help to any reader, if a judicious list of other authorities could have been included.

The Psychological Approach to Religion. By the Rev. W. R. MATTHEWS. London: Longmans, Green & Co. 1925. Pp. 74. 3*s.* net.

Here are three lectures dealing with belief in God, conversion and immortality. They are written in clear and untechnical language, and though they are not remarkably original, they are lucid and attractive. The first lecture endeavours to show that belief in God "is the expression of a need which arises from the instinctive foundations of our life." The second lecture treats conversion as a normal process of striving for unity in life. The third lecture is largely concerned with the problem of the relation of body and mind, and favours an interactionist view.

Conversion: Christian and Non-Christian. A comparative and psychological study. By Professor A. C. UNDERWOOD. London: George Allen & Unwin, Ltd. 1925. Pp. 283. 10*s.* 6*d.* net.

This book merits a much more extended notice than can be given of it here. It is divided into two parts. The first, which is historical, deals fully with conversion as it has been illustrated in the records of many of the important world-religions. The second, which is called psychological, deals with an analysis of the general determining conditions of conversion experiences. The attitude in which the author approaches his subject is thoroughly sound. "The best method," he says, "of studying the psychology of conversion is to examine individual cases that are more or less representative." And he proceeds to justify this statement by an able and fascinating study of a variety of instances. Professor Underwood is alive both to the necessity and to the limitations of a psychological study of conversion. The book is extremely interesting throughout, is well written, and is a distinctly important contribution to a difficult subject.

Immanence and Incarnation. By S. F. DAVENPORT, with a foreword by Dr F. R. TENNANT. Cambridge University Press. 1925. Pp. xxvi + 279. 10*s.* 6*d.* net.

This essay was awarded the Norrisian prize in the University of Cambridge in 1924. It is of philosophical and theological import rather than of psychological interest. After a chapter on "philosophical conceptions," the author endeavours to define as clearly as possible the somewhat fluid notion of "immanence." This he does largely by the help of a critical survey of numerous views which other writers have put forward. He then applies himself to his main task: that of relating the doctrine of immanence to beliefs concerning the personality of Christ.

NOTES ON RECENT PERIODICALS

Zeitschrift für Psychologie.

Bd. 96. Heft 1 & 2.

Über den Bekanntheits- und Fremdheitseindruck. (H. K. Keller.)

The first part of this paper describes and gives the results of experiments on non-recognition (the feeling of the unknown). The chief deductions drawn were (1) that certainty and speed of judgment as to non-recognition were greater the greater the familiarity of similar accompanying objects, even where these known objects did not enter consciousness at all; (2) the experiments are concerned with a comparison by exact methods not of different materials but of different attitudes towards the same material brought about beneath the threshold of consciousness through the knowledge that some objects will be familiar and some unfamiliar, this having its effect on the judgment formed.

The second part endeavours to throw light on the process of recognition. Bourdon tests were used to distract attention from wholes (syllables) and link it to parts (letters); these were followed by tests of recognition and non-recognition of syllables subsequently learnt which had and had not been included in the Bourdon tests. It was shewn that (1) inclusion in the Bourdon test strengthened *recognition*; (2) but without intentional learning and with distracted attention no associations are formed which are of use for *reproduction* with meaningless syllables, but it does occasionally happen with syllables with meaning; (3) mechanical reading aloud if frequent enough (without intentional learning) can help. Hence (*a*) confirmation of the value of motor set; (*b*) support of Müller's and Schumann's theory of stamped-in dispositions of lower motor centres rather than psychical memory (association of kinaesthetic impressions); (*c*) the unnoticed seen object leaves behind a residuum of memory, probably by means of a visual correlate of the already recognised motor-set.

Über die Unterschiedsempfindlichkeit für Beleuchtungseindrücke. (H. Krüger.)

Experiments were undertaken as a contribution to the contested question as to whether illumination is perceived as such or inferred (Helmholtz). Three methods, direct comparison, recognition, determination of the threshold of alteration, all showed that it is more difficult to recognise differences of illumination than to perceive differences in brightness of colour of objects.

Über farbige Umstimmung (Sukzessivkontrast) und Momentadaptation der Hühner. (H. Reichner.)

To carry further research by Katz and Révész (this *Zeitschrift*, 50, p. 93) on the value of permanent coloured lights for adaptation in hens, the author performed a series of experiments on hens which showed (1) the existence of successive contrast for the four chief colours of the colour circle, (2) the presence of simultaneous adaptation (Hering) exactly as in human beings.

Die Rolle von Perseverationen und Bewusstseinslagen bei Willenshandlungen. (A. Prandtl.)

The acts of will here investigated are those which take place after a certain amount of voluntary preparation. The author made experimental tests by filling with an indifferent operation (marking of diagonals in squares) a pause between the setting and the carrying out of a task (making a sentence containing the sounds of three nonsense syllables indicated from a series just learnt). The results showed (1) that not an act of will as such but the additional emphasis on the three chosen syllables caused their more frequent perseveration, (2) the presence of "states of consciousness" (Marbe) in opposition to Müller and Titchener, probably representing a physiological process set at work by the idea of the act to be accomplished, and hence not to be investigated by psychology.

These results the author applies to indicate the greater reliability of the untrained over the trained subject in experimental work, since an intention alone (*e.g.* the intention of closely analysing) can bring about a reaction.

Bd. 96. Heft 3 & 4.

Experimentelle Studien über stereoskopisches Sehen. (W. Schriever.)

The purpose of the author was to carry further the investigation on stereoscopic vision by v. Karpinska and Skubich, and particularly to examine (1) the effect of disparity as such, (2) its influence when associated with the empirical criteria of depth. The most important conclusions were: (1) mixed disparity has a greater effect than crossed or uncrossed, and crossed than uncrossed; (2) the localisation of the point of sight is relative to the objects; (3) disparity always prevails over perspective alone; (4) cross-cutting alone can hinder and even reverse an impression of depth resulting from disparity; half pictures with different cross-cutting only allow 3-dimensional vision when an impression of transparency can be attained.

Die Wahrnehmungszeit. (F. F. Hazelhoff u. Heleen Wiersma.)

The article describes experiments on the determination of speed of perception of light stimuli by the method of localisation, the principle of which is measurement of the amount of error in localisation of a light stimulus appearing in a direction other than that in which the subject's gaze is fixed. These experiments give a perception time of about 0·1 sec. for a light stimulus of about 200 threshold values.

Das aschgraue Mondlicht als Gegenstand der Sinneswahrnehmung. (J. Plassmann.)

An account of a long series of ocular measurements of the brightness of ash-grey moonlight, lumen cinereum, the light reflected from the earth on the dark portion of the young and old moon at twilight, and which to the uninitiated appears less strong than the twilight.

Weitere Beobachtungen über einen Fall von abnormem Datengedächtnis. (R. Hennig.)

Details and instances of the author's extraordinary memory for dates.

Verbreitung und Grad der eidetischen Anlage. H. Zeman.

A series of purely psychological experiments were undertaken in Vienna with the purpose of carrying out Jaensch's suggestion that only experimental research in Vienna on material similar in age and situation and with a similar method to that employed in Marburg could settle the question as to whether difference of material accounted for the difference between the findings of Urbantschitsch and himself.

If "latent" eidetic subjects are included, the findings agree with those of Jaensch, that the eidetic disposition among children of middle school age is nearly universal (91½ per cent. boys, 95 per cent. girls, and quite universal between the ages of eleven and sixteen), and are opposed to those of Urbantschitsch and Fischer-Hirschberg (Breslau). For eidetic subjects in the narrower sense the findings approach those of Kroh (Marburg). Experiments on types of eidetic imagery also support Jaensch. It is suggested that grade be measured by (1) amount of detail, (2) colour, (3) capability of producing the eidetic image with open eyes in a lighted room (only 2½ per cent. could do this with all details), (4) capability of voluntarily drawing the image nearer.

Bd. 96. Heft 5 & 6.

Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrnehmung (1. *Abhandlung*). (W. Ehrenstein.)

The illusions here dealt with are not those of mere movement of lines, but of such movements of lines as involve the illusion of alteration in size of the surface bounded by them, *i.e.* intrafigural movements. The first part treats of these and their accompanying phenomena in various figures, angles, circles, etc., and shows these illusional intrafigural movements to be "the principle cause of the deviation of the psychical from the physical form."

Experiments were made on optimum increase and decrease (25°) dependent both on the size of the angle and on speed of movement, and also to test with logarithmic spirals movement of the after-image. A clear and durable after-image was obtained with these spirals in the vision of the closed as well as of the stimulated eye. Further, "where contours show intrafigural phenomena of movement through shifting of the retinal image, they can also undergo an apparent shifting of position in the direction of these phenomena of movement."

A large part of this section deals with the question of contrast—*i.e.*, apparent movement in one direction brought about as the result of actual movement of some related part in the opposite direction. Some experiments showed contrast as the result of both kinds of movement phenomena, while with other quite analogous stimulus conditions no 'contrast' effects resulted. This difference the author attributes to the influence of 'central' factors.

In the second part, the phenomenon of apparent intrafigural movement is applied in explanation of a series of optical illusions which are increased by eye-movement and lessened or entirely absent with careful fixation.

Experimentelle Beiträge zur Untersuchung des binokularen Sehens. (G. Skubich.)

A careful series of experiments undertaken in order to carry the work of Karpinska on binocular vision a stage further in that actual objects were exposed and correct accommodation ensured by a single fixation point, whereas Karpinska used drawings under conditions which did not allow correct accommodation. The latter was experimentally shown to be an essential condition of correct binocular vision. The enquiry was particularly directed towards the development of 3-dimensional perception, where results tended to support Karpinska's view of its gradual evolution from the perception of flatness. The difference between plastic and distance vision, *i.e.* spatial perception of an object and perception of distances between objects, was experimentally investigated, and seemed to be dependent in part on the difference between the perception of unity and of duality or multiplicity.

Über die sogenannten 'hervortretenden' Farben. (S. Belaiew-Exemplarsky.)

The task here undertaken is that of experimental investigation with a constant reflected light of the apparent greater prominence of some chromatic colours over others. Important factors are proved to be: (1) fixation point; unfixated colours seem nearer and are less definitely localised; hence (in part) the paradox that colours further withdrawn seem with the increasing distance to come nearer; (2) saturation; saturated colours give a 3-dimensional impression and are more easily seen as coloured material or mist; (3) density; thick dense colours (red, yellow) seem nearer as colours than less distinct ones, but the latter seen as coloured light (as coloured empty space) seem nearer than the former, since indistinctness makes impossible definite spatial localisation.

Thus the causes of the apparent shifting of position with different colours are: for all similarly coloured surfaces—vividness and saturation; for different coloured surfaces of equal saturation—vividness and density of colour.

Zeitschrift für Psychologie.

Bd. 94. Heft 1 & 2.

Singen und Sprechen. (C. Stumpf.)

The author concludes that from the acoustic point of view there are no specific fundamental differences between song and speech; but the differences of grade which do exist are so decisive as clearly to differentiate them. The most important lies in the gradual tone changes in speech as contrasted with the use of distinct and disconnected tones in singing.

Über Gegenwartsaufgaben der Jugendpsychologie. (E. R. Jaensch.)

The last of a series of papers "Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter," edited by the present author and forming their preface and introduction in the monographic edition. The writer accentuates the importance for both philosophy and pedagogy of detailed experimental knowledge concerning the method of perception formation in youth and its differences compared with that of the average adult, combating the idea that his work on eidetic types merely exploits the chance discovery of a co-operator.

Die Koordination der Gehirn- und der Bewusstseinsvorgänge. (Antonin Prandtl.)

Both parellelism and the theory of reciprocal action involve (1) a theory of causality for which we have no warrant—we only know sameness, and this only approximately, (2) a trinity in any act of consciousness of (*a*) the reality, (*b*) brain processes, (*c*) perceptions. But it can be shown that there is only a duality, (*a*) the reality, (*b*) the subjective experience of it conditioned simultaneously by *both* physical and psychical factors, and which can be regarded as physical or psychical according to the emphasis given by the point of view.

Bd. 94. Heft 3 & 4.

Die Erregung des Neurons als physiologische Grundlage psychischer Vorgänge. (R. Matthael.)

An account of various observations and experiments showing a connection between neurone activities and psychic processes, as, for example, the changes brought about on the latter by variations in the supply of acid for the former; the way in which activity precedes neurone development; the all-or-nothing reactions of the peripheral part of the neurone; and the graded reactions of the central part corresponding to the strength of stimuli. The latter part of the paper finds explanations of various facts of attention and 'form' (Gestalt) in the arrangement and behaviour of the neurones.

Zur Theorie des binokularen Einfachsehens und verwandter Erscheinungen. (J. Lindworsky.)

The author founds his psychological explanation of binocular single vision on two hypotheses, (1) the normal unity of the individual consciousness, (2) the determination of our knowledge of objects, *i.e.* manifest content, by our sensations in the narrowest sense of the word. We do not distinguish two objects but one, because the same content, indistinguishable to consciousness, is produced by the two excitations (of the two eyes), and touch phenomena produced under similar conditions would produce similar results. The crossing of the optic nerves he inclines to explain on the resonance theory, but not as essential for unity of vision, and he applies the theory derived from the above hypotheses to the problems of the spatial threshold.

Erlebnisanalyse und Sprachwissenschaft. (A. Storch.)

In explanation of the use of the accusative with verbs indicating sense perception Jaensch supposes a corresponding experience, and cites the manner in which the eidetic type alters the position of his visual images in accordance with his wishes with regard to the actual objects; this he believes to be a common stage in the development of perceptions. The author supports this view from both phylogeny and observations on schizophrenics, where there is belief in the power to influence objects directly by means of sight, and where not only does speech show this primitive belief, but words themselves are treated as things.

Neue Untersuchungen über die Zöllnerschen anorthoskopischen Zerbilder. (F. Schumann.)

1. *Die Simultane Erfassung der Figuren.* (H. Hecht.)

In this first of a series of papers on Zöllner's Phenomenon edited by F. Schumann, the author describes a series of experiments in which he discovered the dependence of its appearance on absence of fixity of attention on the slit behind which the figure is moved. He asks three fundamental questions, why (1) the figure seems distorted, (2) its movement appears delayed, (3) it looks broader than the slit, and finds the effects of different speeds and the greater effect on the lateral contours, the resulting figure being a compromise form between that of the object and of the slit,—the nearer the latter the greater the speed. The answers to his questions he sees in the slower movement of the lateral contours of this compromise form. Independence of eye-movement is fully established.

Zeitschrift für Psychologie. I. Abteilung.

Bd. 94. Heft 5 & 6.

Über Grundfragen der Farbenpsychologie. (E. R. Jaensch.)
Der Innere Farbensinn der Jugendlichen in seiner Beziehung zu der angenäherten Farbenkonstanz der Sehdinge. (Oskar Feyerabend.)

Starting from the Hering-Müller theory of antagonistic colour processes and the phenomena of 'transformation' (Jaensch, *i.e.* difference between actual and apparent colour as a result of unusual lighting), which were brought about and tested experimentally, the author was able to establish experimentally a similarity between eidetic and transformation phenomena. He leaves open the question as to whether this similarity be due to identity or to genetic causation, *i.e.*, the genetic relation of eidetic processes to all vision. A further series of experiments was successfully carried out to test the validity of the application by Jaensch to 'transformation' of Pretori and Sachs' 'law' for contrast, "that on a coloured ground (or in a colour-lit space) an area of observation appearing neutral remains neutral when its colour and its white valency increase proportionally." As a result the author considers proven that either (*a*) from the identity assumption: the transformation of colour of abnormally lighted objects is the effect of the same antagonistic process which appears when we have the negative eidetic image instead of the objectively neutral area of observation, or (*b*) from the genetic assumption: the transformation of the colour of abnormally lighted objects is to be derived genetically from the effect of the antagonistic processes which appeared in the eidetic stage of development, and which all eidetic subjects examined, children and adults, showed as complementary eidetic images in the experimental conditions employed.

Studien über Strukturgesetze. (Heinz Werner.)

1. *Über Strukturgesetze und deren Auswirkung in den sogenannten geometrisch-optischen Tauschungen.*

Classifying forms as 'articulate' (gegliedert) and 'unarticulate,' with subheadings for both of 'centered' or 'uncentered,' the author explains optical illusions of the 'geometrical' type on the basis of the two structural laws of assimilation for the latter and dissimilation or contrast for the former. "Differentiation of parts in the articulate figure plays the same function as assimilation for the unity of the unarticulate figure."

2. *Über das Problem der motorischen Gestaltung.*

The above two laws are used to explain differences in the drawing of a figure with closed eyes according as the conception of the figure was of a unity (unarticulate) or of one of a series (articulate). The application to other forms of expression, the *form* of act, is suggested.

Das Urbild. (Hans Henning.)

The author thinks that those senses which have remained in a more primitive stage of development show with greater clearness than the higher senses the occurrence, common to all, of perceptive images (anschauliche Bilder) in an even more primitive form (Urbild) than that of the eidetic image. He finds the sense of smell particularly rich and clear in this direction, and promises further work on other subdivisions of types of imagery.

Experimente an einem telekinetischen Medium. (Hans Henning.)

Untersuchungen an einigen okkultistischen Medien. (Hans Henning.)

An account of the detection by the author of trickery used by telekinetic and occult mediums, followed in the former case by a full account on the part of the so-called medium not only of the apparatus used and the means adopted for its concealment in all similar cases, but also of methods for making of no avail the usual precautions, investigations and tests. The 'medium' emphasised the low opinion held by all of his craft of their 'scientific' investigators, also the alteration in the mental attitude of the latter after the first 'occult' phenomenon. Since these mediums did even more than Herr v. Schrenck-Notzing's mediums in stricter conditions, the author thinks the disproof complete, and that the question for psychological investigation is the state of mind of the credulous.

Über den Rhythmus der russischen Prosa. (Eugen Kagarow.)

A short article giving the results of psychological-statistical experiments on the above.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 24, 1925.	Conational Phenomena in Processes of Judgment, by B. Stevanovic. Apparatus used to Investigate Electrical Charges Accompanying Emotional States in the Insane, by Miss Ethel Stoneman.
December 12, 1925.	Annual General Meeting: A New Method in Mental Testing, by J. J. Strasheim. Eidetic Imagery, by Miss A. M. Jenkin.

SECTIONAL MEETINGS.

Aesthetics Section.

December 11, 1925.	Annual Meeting. Aesthetics and Philosophy, by J. H. Thorburn.

Educational Section.

October 12, and November 9, 1925.	Authority: considered mainly inr elation to the Teacher, by G. H. Green.
December 7, 1925.	Annual Meeting. Some notes on Education in America, by Prof. T. P. Nunn.

Industrial Section.

December 7, 1925.	Annual Meeting.

Medical Section.

October 28, 1925.	(Joint meeting with the Psychiatry Section of the Royal Society of Medicine.) The Early Treatment of Mental Disorders, by Sir Maurice Craig and Dr William Brown.
November 25, 1925.	The Neurotic Character, by Dr Edward Glover.
December 16, 1925.	Annual Meeting. Divergent Tendencies in Psychotherapy, by Dr Edward Glover.

VOLUME XVI APRIL, 1926 PART 4

THREE EXPERIMENTS ON THE LOCALIZATION OF TONES[1]

BY H. BANISTER.

(From the Psychological Laboratory, Cambridge.)

I. *Preliminary* (p. 266).

II. *Experiment to investigate the Effect, on the Binaural Localization of a Tone, of varying the Relative Physical Intensities of the Sound Waves at the Ears* (pp. 266–279).
  (*a*) *Purpose of Experiment* (pp. 266, 267).
  (*b*) *Historical* (pp. 267–269).
  (*c*) *Apparatus* (pp. 269, 270).
  (*d*) *Experimental Method* (pp. 270, 271).
  (*e*) *Observers' Reports* (pp. 272–275).
  (*f*) *Analysis of the Reports* (p. 275).
  (*g*) *Deduction* (p. 275).
  (*h*) *A possible Criticism* (pp. 275–278).
  (*i*) *Remarks* (pp. 278, 279).

III. *Experimental Investigation of the Relative Bilateral Intensities which, the Vibrations being in Phase, produce the same Change in Localization as a Binaural Difference in Phase of $\pi/2$, the Physical Intensities of the Sound Waves at the Ears remaining Constant* (pp. 279–290).
  (*a*) *Previous Work* (pp. 279, 280).
  (*b*) *Purpose of Experiment* (p. 280).
  (*c*) *Apparatus* (pp. 280–282).
  (*d*) *First Experiment* (pp. 282–286).
    (i) *Method* (pp. 282, 283).
    (ii) *Experimental Results* (pp. 283–286).
  (*e*) *Second Experiment* (pp. 286, 287).
    (i) *Method* (p. 286).
    (ii) *Experimental Results* (p. 287).
  (*f*) *Analysis of the Results* (pp. 287–290).
  (*g*) *Comparison with the Results of Stewart and Hovda* (p. 290).
  (*h*) *Conclusion* (p. 290).

IV. *Summary* (pp. 290, 291).
  Appendix (pp. 291, 292).
  References (p. 292).

---

[1] This research was carried out with the aid of a grant from the Medical Research Council.

I. PRELIMINARY.

IN this paper I propose to give an account of three experiments which were carried out with the intention of throwing light on the phase effect in the localization of tones.

The experiments formed part of a programme drawn up with the specific intention of testing the hypothesis, put forward in 1908 by Myers and Wilson(9,14), that the change produced in the apparent localization of a tone, heard with the two ears, when the relative phases of the vibrations of the sound waves at the two ears are varied, is due fundamentally to bilateral differences of intensity.

If the 'phase effect' is an 'intensity effect,' it is of interest to enquire what bilateral difference of intensity, when the vibrations at the ears are in phase, produces the same change in apparent localization as is produced by a given binaural difference of phase between the vibrations, when the amplitudes of the vibrations are kept constant. Also if the 'phase effect' is an 'intensity effect' it follows that, if a tone is localized to one side because the vibration at that side leads in phase, an increase of intensity at the other side, without varying the relative phases of the vibrations at the two ears, will cause the tone to move gradually over to this other side.

The first experiment to be described gives the result of an investigation into the above deduction; the other two experiments show what bilateral ratios of intensity were required to produce the same angular change in localization as was produced by a binaural difference of phase of $\pi/2$.

II. EXPERIMENT TO INVESTIGATE THE EFFECT, ON THE BINAURAL LOCALIZATION OF A TONE, OF VARYING THE RELATIVE PHYSICAL INTENSITIES OF THE SOUND WAVES AT THE EARS.

(a) *Purpose of Experiment.*

If the apparent change produced in the binaural localization of a tone, when the relative phases of the vibrations at the ears are varied, is due to subjective binaural differences of intensity, then any displacement to one side, which is the result of a binaural difference of phase, can be nullified by suitable changes in the relative bilateral objective intensities.

In other words, if the effect of a phase difference between the vibrations at the ears is to produce a subjective increase in loudness at the side which leads in phase, the phenomenal effect of varying the amplitudes of the vibrations will be the same in character whatever the relative phases: it will differ in degree, however, by the amount by which the difference in the phases gives one side the advantage over the other. Also, if a variation in the relative intensities gives one sound with changing localization when the vibrations are in phase, the same general effect should be observed when there is a bilateral difference of phase between the vibrations.

This experiment was designed to test whether the general phenomenal effects of varying the intensities at the ears were independent of the relative phases at the ears, or not.

(*b*) *Historical.*

In 1922, when I was investigating the possibility of using thermionic valves for auditory experiments, my observers repeatedly reported they could hear two sounds, that is, the same sound in two places at once. On examining the circumstances, I found that the 'double hearing,' which I at first ascribed to defects in the apparatus, generally occurred when there was a difference of phase between the vibrations at the two ears. It seemed to me, therefore, that this phenomenon might form the basis of my enquiry, if the experiment were properly controlled.

'Double hearing' has been reported by many observers. Thompson (1877)(13) remarked,

> that when the difference of phase is partial, the sound is heard partly in the ears and partly at the back of the head, the former partial sound dying out and giving place to the latter as the difference of phase approaches a maximum and vice-versa.

And on the next page he states,

> In intermediate positions [*i.e.* where there was a difference of phase at the ears] the effect was of a mixed character: part of the sound was heard as in the ears, part at the back of the head.

'Double hearing' is also reported by Angel and Fite (1901)(1) and Bowlker (1908)(5).

Wilson and Myers ((14), p. 370) also point out that, though it was usual for only one sound to be heard, yet occasionally they could at the same time detect something at the other side.

> Save on one occasion (when it appeared to be a high overtone of the fork) the sound had no musical character whatever.

They continue,

> We cannot offer a satisfactory account of its origin. Perhaps the following observation only serves to increase the difficulty of an explanation. At the scale reading 120, the fork 512 gave 'left, with buzz in right.' But when a resonator was placed between the fork and the T-piece, the sound went over to the right ear.

This 'buzz' was a disturbing factor which they endeavoured, unsuccessfully, to eliminate. It appeared (p. 371)

> not to be confined to a single point of the scale, but to spread over a fairly broad section of it.

When they "proceeded to compare the amplitudes of vibration on the two sides of the apparatus" by means of a manometric flame, they found

> that at certain positions of the T-piece there were considerable differences between the amplitudes of the two flames for the fork 256. The positions of the T-piece, which produced these differences in amplitude, correspond to the positions where it had been observed that the anomalous lateral effects were most marked.

These observations are, in my opinion, very significant. Wilson and Myers themselves, in the extract I have quoted above, confess that they cannot offer a satisfactory explanation.

Later experimenters, Hornbostel(8), Stewart(10), Halverson(7) have remarked on the two sounds sometimes heard. Two sounds are heard, not only when there is a difference of phase between the vibrations at the ears, but also when the vibrations are in phase. Stewart ((10) p. 427), in what he describes as "a brief series of experiments," led sounds to the ears of his observers through rubber tubes, which could be pinched to lessen the intensity. He found observers for whom a variation in the intensity produced, at certain frequencies, no change in the apparent localization of the sound. With a frequency of 1024 d.v./sec. and no difference of phase, he and another observer found that ((10) p. 428),

> when the tube at the left ear was being pinched the single image remained in front until the ratio of the intensities was very large...and there appeared...on the right a second image which increased in clearness with further pinching, the image in front simultaneously disappearing.

In such cases, he remarks,

> one image did not rotate, but remained directly in front, or, by trial, *in the position determined by the difference of phase* [my italics].

Thus, he continues ((10), p. 445), since these observers responded to phase difference at this frequency (1024 d.v./sec.) without any hesitancy, "the effect of phase difference cannot be explained by any indirect 'intensity effect.'"

(c) *Apparatus.*

The apparatus I employed was that used in former experiments (2) and requires no further description. It is shown diagrammatically in Fig. 1. The only change made was that the resistances (*sh*), by which the currents through the telephones were shunted and the intensities of the sounds varied, were replaced by high resistance michrome wire. One of these shunts is shown in Fig. 2. One terminal of the telephone was connected by a copper wire to the end, *A*, of the michrome wire, *AD*; the other terminal was connected to mercury in the U-tube, *BC*, through which the wire, *AD*, passed. The two ends, *A* and *D*, of the wire were joined by a string *AED* which passed over a glass rod *E*. As the end of the wire, *A*, was brought down the length of wire through which the current of the telephone was shunted, was lessened and the current through the telephone decreased, and *vice versa.*

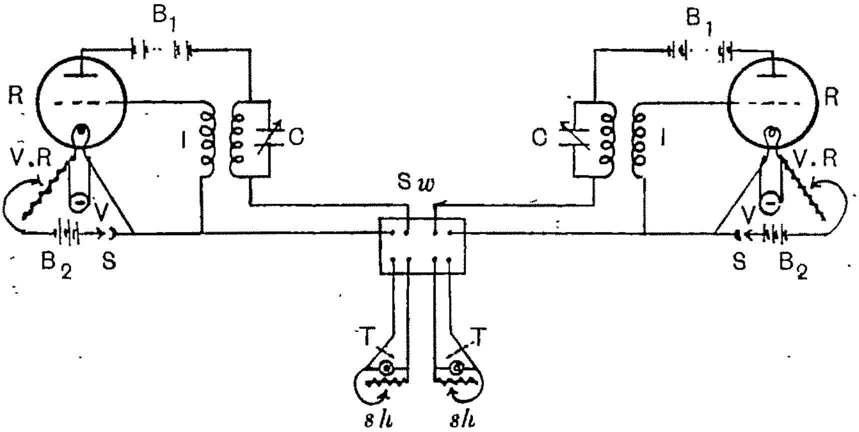


Fig. 1. Diagram of Apparatus.

B_1	high potential battery (54 volts).	*sh*	shunt.
B_2	accumulator (6 volts).	*T*	telephone.
C	variable condenser.	*V*	voltmeter.
I	inductances.	*V.R.*	variable resistance.
R	valve.	*Sw*	4-way switch
S	switch.		

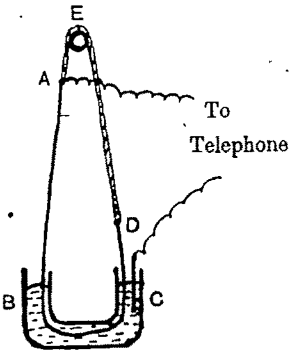


Fig. 2. The Variable Shunt.

AED	string.
AD	michromic wire.
BC	mercury in U-tube.
E	glass rod.

During the experiment I was in the sound-proof room with the observer. The observer sat with his back to the table, facing one wall of the room. I stood on the opposite side of the table, on which was the condenser (*C*), by means of which I could produce any desired phase difference between the vibrations. On the table there were also the switch (*Sw*) for switching the sounds on or off, and the two shunts (*sh*), by means of which the sounds at either side could be cut out altogether, or increased till they were uncomfortably loud. The rest of the apparatus was in an annexe to the sound-proof room. Quantitative measurements of the intensities were not made. The telephones were sometimes put on the head in the usual way, at other times the sounds were led to stethoscope binaurals in the ears through equal lengths of tubing, since some of the observers objected to the headphones. I have examined the two sets of observations: since they do not differ appreciably I am, in what follows, not discriminating between them.

(*d*) *Experimental Method.*

The method of experimenting was as follows:

The observer being seated comfortably was instructed to report if he heard a sound, and if so where. A sound was then given to him. He reported this and usually located it well to one side, the relative intensities at the ears being arranged to give a lateral localization. The intensity of the vibration at the other side was then increased till the observer reported that the sound was at the other side, when the currents to the telephones were cut off. The observer was then asked to describe what he had heard between his first report and the second. No other question was put to him. I wrote down his statement.

The initial physical factors were varied in three ways, *vide* Fig. 3:

(*a*) The vibrations at the two ears in phase, and the sound localized to one side owing to the intensity at the other side being low.

(*b*) The vibrations at the ears differing in phase by $\pi/2$, the sound being localized to the side which was *later* in phase, the intensity at the other side being very low.

(*c*) The vibrations at the ears differing by $\pi/2$ in phase, and the sound being localized to that side which was *leading* in phase, the intensity at the other side being low.

Sometimes the vibration at the right ear led in phase, sometimes that at the left ear led. Examination shows that no useful purpose is served by discriminating between the two sets of results obtained when the vibrations led in phase at the right ear, and when they led at the left.

Each series of observations lasted for about 35 minutes. No fixed number of observations was taken at a sitting.

Eleven observers—6 men and 5 women—took part in the experiment. Not one had acted as observer in any previous auditory investigation. My reason for not using practised observers was that most, if not all, of those who had observed for me previously had occasionally reported hearing two sounds when there was a phase difference between the vibrations at the ears. I thought it possible that they might expect to hear two sounds; while I wished to see if the phenomenon would be observed by those who could have no such expectation. Eight of the observers were students of psychology and were working in the laboratory. I had no fixed number of sittings. Each observer came to me as he was able.

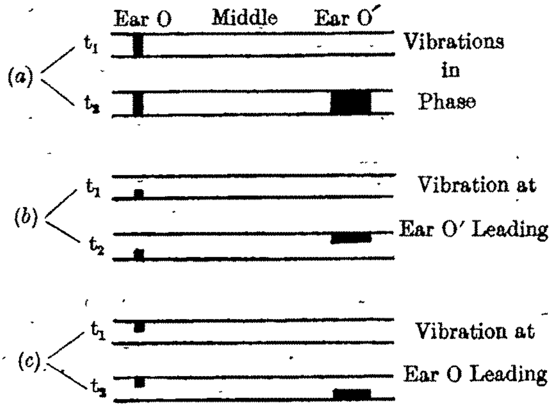


Fig. 3. Diagram of three conditions (p. 270).

The black marks represent sounds in the telephones at the beginning of the observation (t_1), and at the end of an observation (t_2). The intensities are represented by the widths of the marks. The relative phases are represented by the positions of the marks between the lines; the upper mark represents the sound which is leading.

Notes, varying in pitch, from 210 d.v./sec. to 245 d.v./sec. were used at different times.

(*e*) *Observers' Reports.*

The summary of the results of the observations[1] classified according to the number of sounds reported—viz., one sound: doubtful: two sounds—is given in Table I.

TABLE I.

	Physical situation								
				Difference of phase of $\pi/2$ between the vibrations at ears, the sound localized initially to the side					
	(*a*) Vibrations in phase			(*b*) which lags in phase			(*c*) which leads in phase		
Number of sounds Observer	(2) 1 sound	(3) doubtful	(4) 2 sounds	(5) 1 sound	(6) doubtful	(7) 2 sounds	(8) 1 sound	(9) doubtful	(10) 2 sounds
H. 13 sittings	12	—	—	3	—	2	5	4	2
I. 3 ,,	4	—	—	2	—	1	11	—	4
J. 6 ,,	10	1	1	8	—	2	7	2	8
K. 9 ,,	14	2	12	14	1	—	10	4	12
L. 3 ,,	2	2	1	2	1	—	3	3	5
M. 8 ,,	10	6	9	7	1	7	5	4	20
N. 3 ,,	16	—	—	7	1	—	11	2	2
O. 1 ,,	1	—	—	1	—	—	2	1	—
P. 2 ,,	5	2	—	1	1	1	3	—	3
Q. 8 ,,	12	4	10	6	1	4	—	—	19
R. 16 ,,	21	2	3	15	—	3	10	4	9
Total	107	19	36	66	6	20	67	24	84

This table shows the reports of observers classified according as they reported one sound, two sounds, or were doubtful whether one sound or two sounds were heard when the intensity of the sound at one side, originally low, was increased.

The reports of each observer followed the same general course. At first he 'supposed' that the sound moved from one side to the other; then he was sure, he "could follow it round quite easily"; then would come doubt, for "that was different from the previous one in some way"; and the end of the first sitting would usually leave the observer in a very confused and sometimes an unhappy state of mind. He would go away

---

[1] I have omitted one series of observations for Q., because I carelessly omitted to enter on his report when the sounds were in phase, etc. His observations were:

	No. of sounds reported		
Situation	One	Doubtful	Two
(A)	2	—	—
(B)	2	1	—
(C)	—	2	1

I noted shortly afterwards that (A), (B) and (C) were, I thought, what I have designated (*a*), (*b*) and (*c*) p. 270, and a comparison with his other results would seem to bear this out.

wondering whether there was anything wrong with his hearing! It was often not till the second, and sometimes not till the third, sitting that observers would realise that there were differences between the individual observations, and then they would discriminate one from another, and pick out differences, the chief one of which apparently was that sometimes the sound was heard in two places at the same time, and sometimes only in one place—there were either two sounds or one sound. Other differences, variations in loudness, pitch, volume, etc., were noted; but these were not the outstanding differences. The difference, *par excellence*, was that sometimes two sounds were heard and sometimes only one. When one sound was heard, it moved gradually from side to side: when two were heard one stayed still, while another moved over to or appeared on the opposite side, growing louder and louder and eventually, if made loud enough, it drowned the first. H., for example, at her first sitting heard one sound every time; but she had difficulty with one observation. She said,

> That was outside my left ear to start with. I'm not sure where it went, but it arrived at the right front.

At her next sitting, she still reported one sound; but on four, out of five, occasions when the change (*C*) was made she was doubtful. Two of her reports were:

> (*a*) The sound was on the left and filled out towards the back. Then the right began to hear it as well. Finally the sound was all in the right. When I say, "The right ear began to hear it as well," I mean that the sound seemed stationary but the right ear joined in too.
>
> (*b*) The sound seemed to remain in the left for a long time but to go a little higher. Then it began to move—I forget—but I heard with the right ear as well.

The other two reports were similar. The sound was localized on one side, then she "seemed to hear it with the other ear as well." It was most peculiar, she said, to hear the same sound with both ears separately. She went away very perplexed. The next day she came to me saying that she had been rather troubled about what she had heard, but thinking it over she had come to the conclusion that when she said she could "hear it with the other ear as well," she really heard two separate sounds. Five days later she gave me another sitting, and discriminated between the observations—those when she heard one sound; and those when she heard two. The four observations, when H. heard the sound "with both ears," are the doubtful cases I show against her in column 9 of Table I.

Other observers noted a 'peculiar oscillation' of the sound under these circumstances. A typical report, given by W. at her second sitting, is:

> The sound began on the right. It went to the left of the centre, and then back to right of the centre, and then it moved quickly to the left ear.

Such observations, which I have counted 'doubtful' are, I think, comparable to those recorded by Wilson and Myers ((14), p. 371). They state that, where there was a buzz it was possible on one or two occasions "to make the sound appear at either ear by an effort of attention."

It seems to me that the explanation of this phenomenon is, that two sounds were audible, localized in different places, and that, as Myers and Wilson point out, either could be heard "by an effort of attention."

N. reported two sounds twice only. As will be indicated later (p. 279), there were times when he was perplexed and found it difficult to describe what he had heard, but almost at the end of his third and last sitting he reported two sounds, and finished a rather long report with a triumphant "I don't think *that* was imagination." It would appear that he had thought he had heard two sounds before, but that he had put it down to 'imagination' and so had not reported two sounds. This time however he was *sure* there was no imagination—there really were two sounds.

Some doubt may be entertained whether the sound which remained more or less stationary was not some form of after-image. Very little work appears to have been done on auditory after-images, but Bishop (4), who investigated the question in 1921, obtained no evidence of subjective after-images. If these phenomena are auditory after-images, it is difficult to understand why they should appear in one physical situation rather than in another, the sole physical difference between the situations being that in the one the vibrations differ in phase by $\pi/2$ while in the other there is no phase difference between the vibrations. Also, it is scarcely possible to explain the phenomenon in this way, for observers would ask me at times to 'stop,' and would continue listening for some time, the while clearly discriminating between the two sounds and localizing them in different places.

I think it probable that, when observers had once realized that two sounds could sometimes be heard, they occasionally reported two sounds because they 'expected' them. 'Expectation' (though I am not quite sure what to include under the term) does, as I have pointed out elsewhere (2), play an important part in determining what is heard. It may be that expectation was largely the cause of the hearing of two sounds when the relative intensities at the ears were varied, the vibrations being in phase, but I think another explanation possible. Expectation was, without any doubt, a factor at all the first sittings predisposing observers to report one sound when two were actually heard. However, for the purpose of this investigation, I prefer to take the observations as obtained, remembering, however, that they are weighted in favour of the Myers-Wilson hypothesis.

(*f*) *Analysis of the Reports.*

From the data of Table I, we obtain a very definite answer to the question whether the three physical situations are the same subjectively, or whether they are different. A cursory glance will show how very dissimilar the reports on the third situation are from those on the other two, while statistical analysis, *vide* Appendix, more than bears this out. It shows that, if situations (*a*) and (*c*) are to be regarded as the same, the chances of obtaining such a difference between the reports are about 1 in 1,000,000; for (*b*) and (*c*) the chances are also about 1 in 1,000,000; while for (*a*) and (*b*) it is possible that, due to the ordinary fluctuations of sampling, 39 per cent. of the samples drawn from the same 'population' would present as great a difference between them as is shown here.

(*g*) *Deduction.*

We see then that it is practically impossible for the experiences to be identical: the variations are much greater than can be explained by fluctuations of sampling. Since the only difference, physically, between the situations is dependent on the relative phases of the vibrations at the ears, it would appear that the relative phases are not appreciated as binaural differences of intensity.

(*h*) *A possible Criticism.*

It may be asked why there is so little difference between (*a*), when the sounds are in phase, and (*b*) when there is a binaural difference of phase of $\pi/2$ and the sound is initially localized to the side which *lags* in phase, since there is such a great difference between both these cases and (*c*), when the sound is initially at the side which *leads* in phase.

But, if it is assumed that the effect of phase and intensity are in some way, which need not now be specified, appreciated separately, a moment's consideration will show that the effect observed might have been anticipated. To take the last case (*c*) first. We have initially a sound localized, let us say, on the right (Fig. 4 (i), t_1). This sound is due to the right telephone vibrating, while the left telephone is still. Then gradually a sound, of the same pitch as the sound at the right ear, is introduced into the left ear. This sound is later in phase than the sound at the right ear, and, as it increases in intensity, it 'fuses' with the sound at the right ear till—when the loudness of the sound on the left is equal to that on the right—one sound is heard which is localized on the right in the position determined by the relative phases (Fig. 4 (i), t_2).

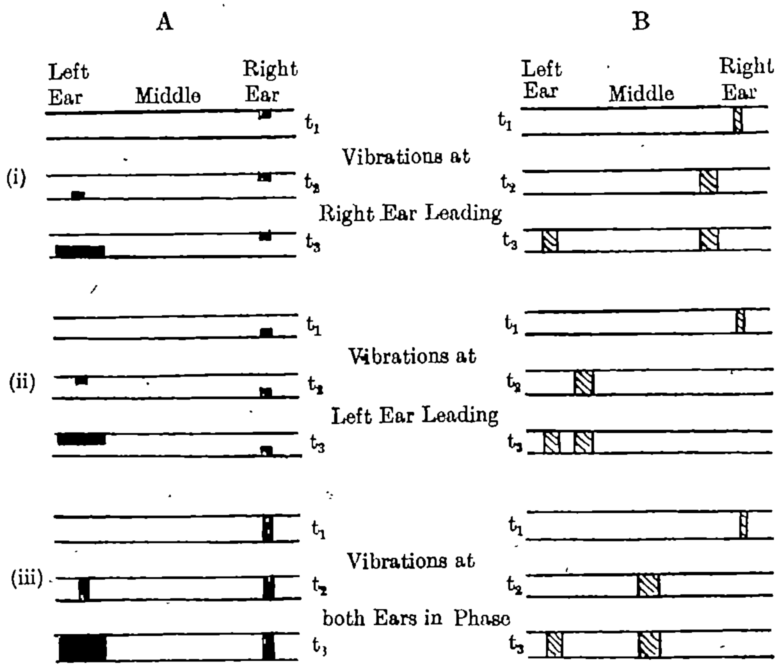

A
B
Left Ear
Middle
Right Ear
Left Ear
Middle
Right Ear
(i)
(ii)
(iii)
t1
t2
t3
Vibrations at Right Ear Leading
Vibrations at Left Ear Leading
Vibrations at both Ears in Phase

Fig. 4. Diagrammatical representation of the physical sounds at the ears (A), and the sounds perceived (B).

(A) show that, at the time (t_1) there is a quiet sound at the right only: at time (t_2) a sound of equal intensity has been brought in on the left: at time (t_3) the intensity of this sound has been increased considerably.

(B) shows the localizations of the sounds perceived.

A further increase of the intensity in the left telephone now affects that ear only since it has no corresponding sound on the right with which to fuse. When this excess of energy has reached a certain amount, it is perceived as a second sound (Fig. 4 (i), t_3), which increases in loudness, as the intensity is increased, till it may eventually drown the sound on the right.

In the other case, (*b*), we have a sound localized, let us say, on the right as before; and no sound on the left (Fig. 4 (ii), t_1). Then the left telephone is made to vibrate, so that the left ear is now subject to sound vibrations of gradually increasing intensity, their phase being in advance of that of the vibrations at the right ear. The result is that a portion of the sound on the right 'fuses' with the sound on the left, being perceived as a sound localized on the left (Fig. 4 (ii), t_2). The loudness of this sound is, perhaps, at first, not sufficient to be appreciated—the first phenomenon to be noticed would probably be a decrease in the loudness on the right. This decrease would continue while, at the same time, and by corresponding degrees, another sound would gradually grow up on the left. This decrease at one side and increase at the other might very easily produce the impression that there was one sound only, and that it moved gradually over from right to left.

As the intensity on the left was still further increased two sounds, as at Fig. 4 (ii), t_3, would be on the left. They might, or might not, be localized together; but it would seem probable that they would be perceived as one sound having a certain localization, which would approach more and more to the aural axis as the intensity at the left was increased. The essential difference between the two cases is that, in the former, making the left telephone vibrate would *increase* the loudness of the sound on the right; in the latter it would *decrease* the loudness on the right.

If we consider case (*a*), where the vibrations are alike in phase, we see (Fig. 4 (iii)) that the result of increasing the intensity on the left would cause a sound to be heard in the median plane while the sound on the right died away. Thereafter another sound would arise at the left. The phenomenal effect of this would be similar, one might expect, to that of case (*b*), Fig. 4 (ii). The sound, originally on the right, would appear to move over to the left. If this explanation is correct it would seem probable that the chances of two sounds being discriminated would be greater when the vibrations are in phase (Fig. 4 (iii)) than when the vibration at the left ear was leading (Fig. 4 (ii)), because of the greater distance between the localizations of the two sounds. The data of Table I bear this out, for, dividing the doubtful cases equally between the 'one' and 'two sounds' we have two sounds reported on 28 per cent. of the occasions when the vibrations were in phase, and on 25 per cent. of the occasions corresponding to the vibrations at the left ear leading.

It would be interesting to test this suggested explanation by comparing the discrimination of the two sounds with various phase-differences and with tones of different pitch—an enquiry I hope to make in the future.

(*i*) *Remarks.*

It will be remembered that Wilson and Myers reported hearing a 'buzz' on certain occasions when the phase-effect was complicated by intensity effects (p. 268). Such 'noises' were sometimes reported in my experiments. They were described in various terms. One observer called it a "buzz," another a "sizzling," a third a "wheezy sound," and a fourth "a sound like a draught." These were the sounds which "came in on the other side" and gradually became tones, when their intensity had been increased sufficiently. This is the effect which was observed, I suggest, by Myers and Wilson, and it is borne out by the observation (*vide* p. 268) that, if a resonator were interposed between the fork and the T-piece the sound went over to the side on which the buzz was heard.

This phenomenon is worthy of further investigation. It might throw light on the much debated theory of audition in general. It is peculiar, if it is a fact, that, when the auditory end organs are being stimulated in a certain way by a periodic force, a tone is localized to one side while a 'noise' creeps into consciousness at the other. It may be that the noise is objective so far as the ear is concerned, that, whatever the mode of excitation of the end organ, the conditions which produce the extraneous noise are in some way due to non-harmonic vibrations set up in the end organ itself—the end organ, already vibrating in one way, being forced to vibrate in another way also, and that the 'buzz' is the perception of this 'forcing' which, before it reaches a steady stage, manifests itself objectively simply as an interference effect having no special frequency. On the other hand, the 'noise' may be subjective. The second sound may be coming in as a tone, but being unexpected and alien to the situation, the judgment may be made that it is a foreign sound altogether—a noise. Or, again, it may simply be that sounds of very low intensity have no pitch value.

In this connection the following case is of interest. Observer N., whom I have mentioned before, only came three times. For one observation at his second sitting the sounds in the telephone were :

(1) right only; and then

(2) a gradual increase in the left.

The vibrations at the right ear led in phase.

His report was

That sound was low right to begin with. It seemed to die away. It sank down on the right and, as it went, I had a sort of sinking feeling as in a lift. I then heard another sound to the left.

So we have the peculiar fact that, though the total intensity was gradually increased the sound seemed to die away, and to move further and further away. The apparent movement was accompanied by a 'sinking feeling,' and then another sound was heard on the left.

This is the only occasion, in my experience, when any feeling, in any way allied to nausea, has been reported. Dr Hartridge has suggested (personal communication) that a feeling of nausea is to be expected when the end organs are stimulated in a way to which they are not accustomed. This may be the explanation of N.'s 'feeling.' It will be recalled that it was this observer who was so sure (p. 274), at his next sitting, that two sounds he reported were not the outcome of his imagination.

III. EXPERIMENTAL INVESTIGATION OF THE RELATIVE BILATERAL INTENSITIES WHICH, THE VIBRATIONS BEING IN PHASE, PRODUCE THE SAME CHANGE IN LOCALIZATION AS A BINAURAL DIFFERENCE IN PHASE OF $\pi/2$, THE PHYSICAL INTENSITIES OF THE SOUND WAVES AT THE EARS REMAINING CONSTANT.

(a) *Previous Work.*

Little quantitative work has been done on the effect of bilateral intensity differences on the localization of tones. Stewart and Hovda (12) found that, for a tone of 256 d.v./sec., a variation of the relative intensities at the ears, the vibrations being in phase, caused the sound to move from a median plane position, which it occupied when the intensities were equal, towards the side of greater intensity, and that, for any given values I_R and I_L of the intensities at the right and left ears, the angular position, θ, of the sound was given by the formula

$$\theta = K \log \frac{I_R}{I_L},$$

where K was a constant for the individual.

The investigation was continued by Stewart, who published the results a few years later, in 1920–1922 (10, 11). He found the law held for frequencies 256 (two observers), 512, and 1024 d.v./sec. (three observers); but that the value of K was different for the three individuals, and that it decreased with increase in pitch.

Further *qualitative* experiments on 16 observers with frequencies of

200 to 2,000 d.v./sec. indicated that the effect of increasing the intensity at one ear did not always produce a displacement of the sound; instead a second sound was found to appear at certain frequencies with certain individuals.

The value of K obtained for the two observers with frequency 256 d.v. was 16° and 30°. For the first of these observers we see from the Fig. on page 436 of the *Phys. Rev.* xv, 1920 (10), that a phase difference of 90° at the ears produces an angular displacement of $90°/1{\cdot}6 = 56°{\cdot}25$. So, for this observer, the ratio of the intensities required to produce a displacement equal to that which is produced by a difference of phase of $\pi/2$ at the ears, is given by

$$56°{\cdot}25 = 16 \log \frac{I_R}{I_L},$$

or

$$\log_e \frac{I_R}{I_L} = \frac{56°{\cdot}25}{16} = 3{\cdot}5,$$

or

$$\frac{I_R}{I_L} = 33.$$

Stewart and Hovda used a tuning fork as the source of sound. Two tubes, connected to binaurals, were placed near the same prong of the fork, and intensity differences were obtained by moving one tube nearer to, or further from, the fork. The result was that, as the intensity of the sound to one ear was increased (or decreased), the phase of the vibration at that ear was made to lead on (or lag behind) that at the other ear. The error due to this cause was, as they point out, probably extremely small. It would tend to minimise the intensity ratio.

(b) *Purpose of Experiment.*

It seemed desirable that the experiment should be repeated with other apparatus and with different methods. This I have done. By two different methods I have found for a given tone what objective bilateral intensity ratio produced the same localization as a binaural phase difference of $\pi/2$.

(c) *Apparatus.*

The apparatus I used was that already referred to, p. 269. The only change in the apparatus, which is shown diagrammatically in Fig. 5, was that a galvanometer and milliammeter (G) were added and connected in series with the telephones, to measure the currents through the telephones. The intensities of the sounds at either ear could be varied by altering the michrome wire telephone shunts (*sh*), and the relative phases of the vibrations could be controlled by the condensers.

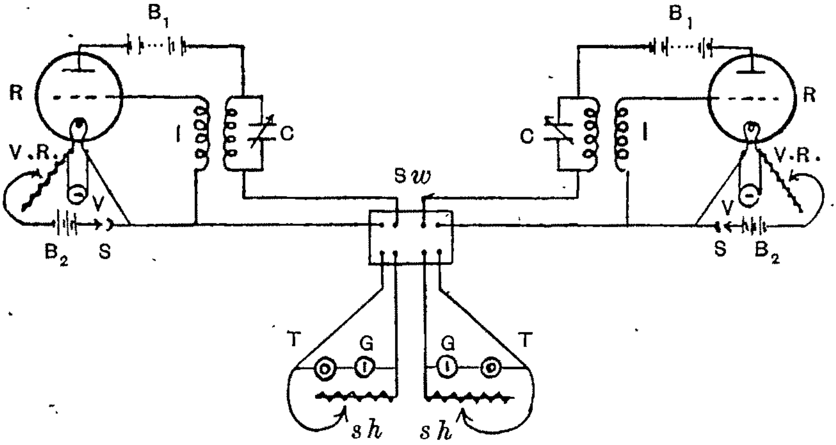


Fig. 5. Diagram of Apparatus.

B_1	high potential battery (54 volts).	S	switch.
B_2	accumulator (6 volts).	*sh*	shunt.
C	variable condenser.	T	telephone.
G	milliammeter.	V	voltmeter.
I	inductances.	$V.R.$	variable resistance.
R	valve.	*Sw*	4-way switch.

The critical matter in an investigation involving intensities is the measurement of the intensities used. In the *Dictionary of Applied Physics* (6) and in Barton's *Sound* (3) the relation between the current through a telephone and the amplitude of the vibration of the diaphragm, considered as a particle at the centre of the diaphragm, is

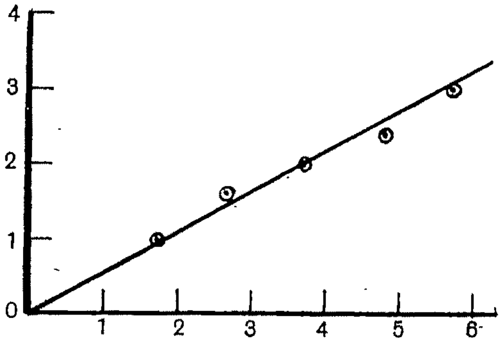


Fig. 6. Curve showing the relation between the amplitude of the vibration of a telephone diaphragm and the current through the telephone.

given. It is shown that the amplitude varies directly as the current. This is generally considered sufficiently exact for most purposes, but I wished to verify the calculations and so took photographs of the vibrations with various strengths of current, mounting mirrors for the purpose as described fully in a previous paper (2). A typical curve, giving the relation between the current strength and the amplitude of the vibrations is shown in Fig. 6. It is sufficiently conclusive proof that the theoretical calculations are correct within the limits required by this investigation. For the purpose of the experiment, therefore, we may consider that the amplitude of the vibration of the diaphragm is proportional to the mean current passing through the telephone.

During an experiment the subject was in a sound-proof room, seated at a table on which were a form for his report and a microphone by which he could communicate with the experimenter. The experimenter was outside, in an annexe, with all the apparatus excepting the headphones. A speaking tube was also provided, so that, in case of necessity, the experimenter could speak to the observer.

(*d*) *First Experiment.*

(i) *Method.* The two methods, generally known as the Methods of Production and Presentation[1] (or 'Constant Method'), were used. In the former (Production) the procedure was as follows:

The observer being comfortably settled down said, "Ready," when the tones were switched on, the vibrations being in phase with one another. The experimenter then, in conformity with the instructions of the observer, arranged for the fused sound so heard to be located in the median plane. This was done by increasing or decreasing the intensity of the current through one or other of the telephones. The sound having been centred, the current was switched off, and the experiment proper began. After a short interval the switch was again closed, the observer localized the sound and if it were not in the median plane, instructed the experimenter, who made any readjustment necessary to get it into the centre. The observer then reported "Centre," and listened. The experimenter produced a phase difference of 90° between the vibrations in the telphones, by varying the capacity of one of the condensers, and after about 2–3 seconds switched the sounds off. The observer noted down the angle through which the sound appeared to move, said "Ready" and the observation was repeated, with these modifications:

---

[1] Cf. Myers' *Text Book of Experimental Psychology*, I, chap. xv.

(*a*) that the experimenter produced the change of direction by increasing or decreasing the intensities of the currents in the telephone, the vibrations being always in phase; and

(*b*) that the observer directed the experimenter to move the sound 'right' or 'left' till it was localized as nearly as possible in the position finally reached by the 'standard' sound.

Observers were given no information whatever. They did not know how the changes in localization were produced.

To compensate for any possible differences in the telephones they were always changed over half way through each series of observations; and to ensure that it was the 'phase effect' which was being observed the switch (*Sw*) was varied at random, so that for one observation the right telephone lead in phase, for another the left telephone lead. Ten such pairs of observations were taken at one sitting, which lasted for about 30–40 minutes.

Observers were asked to write down the angle through which the sound appeared to move, simply as a guide to help them in their matching. They were informed that the one thing of importance was that they should get the second of each pair of sounds to the same position as the first; and, when some remarked that the standard sound did not always move by the same amount, they were informed that that did not matter so long as the second of each pair was brought to the same place as the first.

Various tones were used. Their pitches varied from about 220 d.v./sec. to 300 d.v./sec. An examination of the results shows that this variation in pitch does not produce any significant variation in the required intensity ratio.

Six observers, 2 women and 4 men, took part in the experiment. Four (A., C., D. and F.) were co-workers in the Psychological Laboratory; but only one, D., had observed for me previously. One of the two outside observers, B., had also acted as observer for me before.

(ii) *Experimental Results.* I again found, both in this experiment and in the one next to be described, that there was no constancy in the judgments by the individual of the angle through which a given phase difference caused the sound to appear to move: but there was a fairly close agreement in the changes in the relative intensities at the ears which would produce the change of direction—a very much closer agreement, as will be seen later, than might have been expected.

Yet it was not always the largest difference of intensity which produced the largest angular change in the direction of the sound. The following eight successive observations by A., made at one sitting, show the kind of variation which was very common.

A difference of phase of $\pi/2$ at the ears caused the sound to appear to move from the median plane, and to take up, relatively to that plane, a position at an angle of ...	90°	90°	80°	70°	50°	60°	50°	60°
Percentage increase of amplitude on one side which produced the change in direction noted above	142	200	315	200	250	233	214	230

I give them since on each occasion the intensity was increased.

In the following nine observations (observer C.) the movement of the second of each pair of observations was produced sometimes by increasing the intensity and sometimes by decreasing the intensity.

A phase difference of $\pi/2$ at the ears caused the sound to appear to move from the median plane and to take up, relatively to that plane, a position at an angle of		75–80°	90°	90°	80–90°	90°	75–80°	90°	nearly 95°	50°
Percentage change in amplitude on one side which produced the change in direction	Increase	—	250	215	—	—	150	177	—	—
	Decrease	65	—	—	53·5	37·5	—	—	28·5	25·0

It will be seen that on four occasions the movement was produced by increasing the intensity on one side. For three of these four observations the sound appeared to move through an angle of 90° while on the fourth it moved through 75° to 80°. On this last occasion the increase in intensity was less than that required for the other three movements, though the difference is not so great as the variations in the amounts required to produce the constant angular movement of 90°. If, however, we turn to the cases where the intensity to one side was decreased, we see that the largest difference, when the amplitude of the vibration to one side was decreased to $\frac{1}{4}$, was required when the angular movement, estimated to be 50°, was the smallest.

In working out the results, I have neglected those observations when the observer judged that the standard sound had moved to the side which was later in phase. These errors of judgment were rare.

A summary of the report is given in Table II. This table shows the percentage increase or decrease in the amplitude of the vibration at one side, the vibrations at both ears being in phase, which was found to cause the same angular change in the localization of the sound as was produced by the phases of the vibrations being made to differ by $\pi/2$, the amplitudes being kept constant.

TABLE II.

Observer	Percentage increase of amplitude		Percentage decrease of amplitude	
	Mean	p.e. of mean	Mean	p.e. of mean
A.	219·5	6·9	43·9	3·4
B.	298·5	19·2	29·1	2·9
C.	213·6	14·4	45·9	3·41
D.	162·2	8·06	44·4	3·37
E.	158·1	2·96	34·8	4·4
F.	203·2	7·8	45·7	2·5

This table shows what percentage increase or decrease of amplitude of vibration on one side was required, when the vibrations were in phase, to cause a fused sound to take up the same angular position, with reference to the median plane, as was taken up by the fused sound when the amplitudes were kept constant and the relative phases were changed from 0° to $\pi/2$.

The significance of these results is, perhaps, most easily grasped if the mean values are shown as proportionate increases or decreases, as in Table III. It will be seen that the proportionate increases and decreases of intensity, which are obtained by squaring the amplitudes, agree very closely for observers A., B., C. and F.; the other two observers require on the average a greater decrease than increase of intensity to one side to produce the change in the position of the sound. This tendency to demand a greater decrease than increase in intensity is common to all the observers, though it is much the most marked with D. and E. It would be interesting to follow this clue further to see whether this is an after-effect of the louder sound or not, and if so to what it is due; for with the comparatively few observations so far obtained it would be unwise to generalize.

TABLE III.

Observer	Proportionate change in amplitude on one side	
	Increase	Decrease
A.	(2·2)	(1/2·3)
B.	(3·0)	(1/3·4)
C.	(2·1)	(1/2·2)
D.	(1·6)	(1/2·3)
E.	(1·6)	(1/2·9)
F.	(2·0)	(1/2·2)

Proportionate increase and decrease of amplitude on one side which produced the same angular change of position from the median plane as was produced by a change of phase of $\pi/2$.

The least difference of intensity required for the change—observer E. increase—is that produced by an increase of amplitude of approximately 1·58. If we take the initial amplitude at each ear as unity, then, when the amplitude at one side has been increased in the proportion of 1 to 1·58, the intensity at that side will have been increased from unity to $(1{\cdot}58)^2$. That is, it will be $(1{\cdot}58)^2$ ($= 2{\cdot}496$) times the initial intensity.

So the minimum binaural ratio of intensity required by any of my five observers to produce the change in localization is 2·496. The maximum change of intensity ratio—observer B. decrease—is

$$\left(\frac{1}{3{\cdot}436}\right)^2 = \frac{1}{11{\cdot}80}.$$

A binaural ratio of 11·80 : 1.

(*e*) *Second Experiment.*

(i) *Method.* For the second experiment, by the Method of Presentation, the apparatus was arranged exactly as for the Method of Production (p. 280, *et seq.*).

The experimental procedure was also very similar. The only difference was that, after the sound had been 'centred,' the experimenter caused its localization to change by producing either an intensity difference or a phase difference at the ears, and the observer reported whether the second sound of each pair moved through a greater, equal, or smaller angle than the first.

To help in forming his judgment the observer recorded the angle through which each sound moved. He was, however, given clearly to understand that no value was to be attached to the actual magnitudes of the angles, and that the values noted would be considered as having reference to each pair only and as furnishing the judgment of greater, equal, or less.

There are two advantages in this method of experimenting. The first is that the 'time error' is obviated, for the 'standard' may be presented first or second at random; the second is that the experimenter is not so much in evidence, for, instead of carrying out the instructions of the observer, he varied the relative intensities and phases as he wished and the observer recorded the consequent changes in localization. So the unknown and variable factor, due to the speed with which the experimenter complied with the observer's wishes, was eliminated. But there are corresponding disadvantages in the method, the greatest of which, perhaps, is that a very much larger number of observations must be taken if accurate results are to be obtained.

Observers were informed that each pair of sounds would move in the same direction. Twice, however, this was not observed. These results have been omitted.

As before the telephones were changed over half way through each series of observations, and the sound was made to move to the right or to the left at random.

Ten pairs of observations were taken at a sitting, the length of which was usually not more than half an hour, and four series of observations were obtained from each observer. The tone used had a frequency of 220 d.v./sec.

Of the six previous observers only four (A., C., D. and E.) were available; one woman and one man had left Cambridge. However, both of those who were most sensitive to the intensity difference, *i.e.* those who required the *least* difference of intensity to produce a given angular change of position (D. and E.) were included.

(ii) *Experimental Results.* The results are given in Table IV. The figures show the percentage number of occasions on which the movement produced by the standard sound (the phase effect) was greater than that produced by the intensity difference. For example, the 85 in the row for observer A. indicates that the angular change in position produced by the 'phase effect' was judged to be greater than the angular change in position produced by increasing the amplitude of the vibration at one side to between 151 per cent. to 175 per cent. of the initial amplitude on 85 per cent of the occasions on which this increase of amplitude was made.

TABLE IV.

	Percentage decrease of amplitude to between				Percentage increase of amplitude to between				
Observer	0/25	26/50	51/75	76/100	101/125	126/150	151/175	176/200	201/225
A.	—	—	100	—	100	25	85	50	37·5
C.	33	58	73	—	100	67	75	93	83
D.	—	—	—	—	38	35	25	33	0
E.	—	—	—	—	75	68·7	53	50	20

Table showing the percentage of the occasions on which the angular change in localization, produced by a phase difference of $\pi/2$ at the ears, was greater than the angular change in localization, produced by increasing or decreasing the amplitude of the vibration at one side to a certain percentage of the initial amplitude.

N.B. Judgments of equality have been equally divided between Greater and Less.

(*f*) *Analysis of the Results.*

It will be seen that observer A. judged that an increase in amplitude at one side, of between 1·76 and 2·00 times the original amplitude, produced the same angular localization of the sound as a phase difference of 90° at the ears[1]. This value agrees fairly closely with that she obtained by the method of Production, viz. (2·19) Table II. Observer E. also gives a result (1·76 to 2) which agrees with his previous one of 1·58.

Observer C. reported that a phase difference of $\pi/2$ produced on the whole a greater change of localization than any intensity difference, due to *increasing* the amplitude of the vibration on one side, which was given him. The greatest increase of amplitude which he was given was 2·17 times the initial amplitude. This difference of amplitude, he reported, produced the same angular movement of the sound as the phase difference of $\pi/2$. This closely agrees with the value of 2·14—the ratio of the amplitude which he required to produce the change of position by the method of Production (Table II). It was not convenient for me to produce a greater difference of amplitude than about 2·1 by increasing the intensity to one side, and also this observer, at his first sitting, noted the increase in the loudness of one of each pair, marked the one which did not change in this way and told me, after the sitting, that he had been able to discriminate the standard from the variable—and he had done it correctly. (None of the other observers made such a discrimination throughout the experiment, though changes in loudness were noticed.) I therefore, in the succeeding sittings, produced the intensity differences, for this observer, sometimes by decreasing and sometimes by increasing the amplitude of the vibrations. So that, though he occasionally "tried to discriminate" he "had to give it up."

The decrease in amplitude to one side, which produced the required change in angular position, is between 25 per cent. to 50 per cent.—as a matter of fact 83 per cent. of the observations in this class were judged to produce an angular localization equal to that produced by the change in phase of $\pi/2$. The corresponding decrease of amplitude found by the method of production was 45·9 per cent.

The fourth observer, D., however, reported that, whatever the intensity difference might be, it produced on the average a greater change in the angular localization of the sound than the phase change of $\pi/2$ caused; whereas, by the other method (of Production) the amplitude of the vibration to one side had to be increased 1·62 times to obtain the same angular localization.

It is possible that a larger number of series of observations might have elicited data which would have corroborated his previous result. But that is doubtful, and it was not possible for the observer to give me more of his time. He had already done a considerable amount of listening for me, having acted as observer during the last three years, and was more erratic in his judgments of the angular change in localization than other observers. His forty judgments of the change in direction produced by the phase effect were:

---

[1] The figures for an increase to between 1·26 to 1·50 times may, I think be omitted, since only two observations were taken with this binaural difference.

A change of phase of $\pi/2$ caused the following apparent angular changes in the localization:

Angular change of localization	90°	85°	80°	75°	70°	65°	55°	40°	35°	30°	20°	Total
No. of times this angular change was reported	10	7	10	1	5	1	1	1	1	1	2	40

The judgments show no regularity in their variation. They appear to be quite at haphazard, with a concentration round 80° to 90°.

Similarly, intensity differences appeared to cause movements bearing little or no relation to the relative intensities. The reports were:

	Relative increase of amplitude of vibration at one side from 1 to between					
Change produced in angular localization	1·0 and 1·25	1·26 and 1·50	1·51 and 1·75	1·76 and 2·00	2·01 and 2·75	Totals
90°	2	7	10	1	2	22
85°	1	1	4	1	—	7
80°	1	3	2	—	—	6
70°	—	—	—	1	—	1
60°	—	—	1	—	—	1
45°	—	2	1	—	—	3
Totals	4	13	18	3	2	40

The figures in the above statement show the number of times a given increase of amplitude was judged to produce the angular change of position shown in the first column.

A division of the judgments according as the movement was to the right or to the left also shows no relation between the magnitude of the angles and the direction of the movement.

His introspections are, however, of interest. They disclose the fact that this observer was paying attention to the 'volume' of the sounds, to apparent variations in pitch, and to the manner in which the sounds appeared to move. All these are matters of psychological importance, nevertheless, I do not propose to deal with them in this paper.

These interests may have affected his judgments, which, as I have shown, appeared to be very much at random; and, since the character of the experiment was so different from that of the method of Production, where he was required actually to 'match' the angles, this experiment could not, perhaps, in this particular case, be expected to give such definite results.

The findings of this observer being, in this particular experiment, of a negative character cannot however be considered as vitiating the general conclusion, that this experiment furnishes strong corroborative evidence for the fact that with tones of about 250 d.v./sec., when the vibrations at the two ears are in phase, a very considerable difference in the physical intensities at the ears is necessary if the direction of the sound is to be the same as it would be if there were a difference of phase of $\pi/2$ at the ears, the physical intensities being kept constant.

In no instance, excluding the negative results of D., was the required difference in intensity less than that previously obtained (2·49)—corresponding to an increase of amplitude of 1·58 times the initial amplitude—for E., while the correspondence between the results of the two experiments is very close, considering the difference in the methods, and the comparatively few observations taken by the method of Presentation.

(*g*) *Comparison with the Results of Stewart and Hovda.*

It will be seen that the effect of binaural intensity differences was much more marked with my observers than with Stewart's and Hovda's. Whereas, for the one observer for whom Stewart gives data which can be compared with mine, the intensity ratio for a frequency of 256 d.v./sec. was 33 : 1, for my six observers the maximum was 11·8 : 1 while the minimum was 2·5 : 1. It is difficult to say whether this is wholly an individual difference, or whether it may not, in part at least, be due to the difference in the methods employed.

(*h*) *Conclusion.*

It would be of interest if further results by these, or other, methods were obtained. In the meantime it seems probable that a minimum objective bilateral intensity ratio of at least 2·5 : 1 is needed to produce the same angular localization of a tone, of about 220 to 300 d.v./sec., as is produced by a binaural phase difference of 90°.

The bearing of this result on the Myers-Wilson hypothesis will be dealt with elsewhere.

IV. SUMMARY.

In this paper I describe the results of an investigation into the phenomena to be observed, when the ears are stimulated simultaneously, but separately, with tones of the same pitch, about 210 to 245 d.v./sec. According to the Myers-Wilson hypothesis the phenomenal experiences should, in general, be the same when, one ear being stimulated by a constant sound, the intensity at the other ear is increased: the experiences should not vary in nature as the relative phases of the vibrations at the ears differ. This was not found to be the case. Two sounds were clearly discriminated when there were certain relations between the amplitudes and the phases of the vibrations at the ears. With other relations between the phases and intensities such a distinct discrimination was not made. The localizations of these sounds would appear to indicate that binaural phase differences are appreciated in some manner which is distinct from the appreciation of binaural intensity differences.

In a further investigation I determined the relative bilateral intensities, the vibrations being in phase, which would produce the same angular localization of a tone, with reference to the median plane, as a binaural phase difference of $\pi/2$ at the ears, the intensities being kept constant. For six observers, with tones of about 220 d.v./sec. to 300 d.v./sec. the minimum intensity ratio was found to be 2·5 : 1; the maximum 11·8 : 1. The significance of this result, so far as it applies to the Myers-Wilson hypothesis, will be dealt with in another paper.

APPENDIX.

CALCULATION OF THE PROBABILITY OF THE VARIOUS OBSERVATIONS, PARA. 27, BEING DRAWN FROM THE SAME 'POPULATION.'

The (a) and (b) observations.

Situation	No. of sounds reported			Total
	One	Doubtful	Two	
(a)	107·00 110·34 −3·34	19·00 15·94 +3·06	36·00 35·72 +·28	162
(b)	66·00 62·66 +3·34	6·00 9·06 −3·06	20·00 20·28 −·28	92
Total	173	25	56	254

Whence $X^2 = 1{\cdot}89$ and $P = {\cdot}39$.

For (a) and (c) observations.

Situation	No. of sounds reported			Total
	One	Doubtful	Two	
(a)	107·00 83·64 +23·36	19·00 20·67 −1·67	36·00 57·69 −21·69	162
(c)	67·00 90·36 −23·36	24·00 22·33 +1·67	84·00 62·31 +21·69	175
Total	174	43	120	337

Whence $X^2=28{\cdot}52$ and $P={\cdot}000001$.

For (b) and (c) observations.

Situation	No. of sounds reported			Total
	One	Doubtful	Two	
(b)	66·00 45·83 +20·17	6·00 10·33 −4·33	20·00 35·84 −15·84	92
(c)	67·00 87·17 −20·17	24·00 19·66 +4·33	84·00 68·16 +15·84	175
Total	133	30	104	267

Whence $X^2=27{\cdot}1$ and $P={\cdot}000001$.

[N.B. The upper figures in each of the central compartments are the actual observations; the middle ones are the 'independence frequencies'; the lowest ones are the differences.]

REFERENCES.

(1) Angel and Fite. *Psych. Rev.* VIII, 1901, 225–246.
(2) Banister. *Brit. Jour. Psych.* XV, 1925, 280–307.
(3) Barton. *Text Book of Sound.*
(4) Bishop. *Amer. Jour. Psych.* XXXII, 1921, 305–325.
(5) Bowlker. *Phil. Mag.* (6), XV, 1908, 318–332.
(6) *Dictionary of Applied Physics*, II, 846.
(7) Halverson. *Psych. Mono.* XXXI, 1922, 7–29.
(8) Hornbostel. *Psych. Forsch.* IV, 1923, 64–114.
(9) Myers and Wilson. *Proc. Roy. Soc.* LXXX, A., 1908, 260–266.
(10) Stewart. *Phys. Rev.* XV, 1920, 425–445.
(11) —— *Psych. Mono.* XXXI, 1922, 30–44.
(12) Stewart and Hovda. *Psych. Rev.* XXV, 1918, 242–251.
(13) Thompson. *Phil. Mag.* (5), IV, 1877, 274–276.
(14) Wilson and Myers. *Brit. Jour. Psych.* II, 1908, 363–385.

(*Manuscript received* 12 *July*, 1925.)

THE EFFECTS ON VISION OF REPLACING CONTINUOUS BY FLICKERING ILLUMINATION[1]

BY H. HARTRIDGE, R. J. LYTHGOE AND W. V. MATTHEWS.

(*From the Physiological and Psychological Laboratories, Cambridge.*)

1. *Introduction* (pp. 293, 294).
2. *The Light Sources employed* (pp. 294, 295).
3. *Visual Acuity* (pp. 295, 296).
4. *Stereoscopic Vision* (pp. 296–298).
5. *Achromatic Vision* (p. 298–300).
6. *Chromatic Vision* (pp. 300–302).
   (*a*) *Colour Matching* (p. 300).
   (*b*) *Colour Naming* (p. 300).
7. *Appreciation of the Colours of Small Objects* (p. 302).
8. *Peripheral Colour Vision* (pp. 302, 303).
9. *Perception of Movement* (pp. 303, 304).
10. *Effects on the Pupil Reflex* (pp. 304, 305).
11. *Speed of Reading* (pp. 305–309).
    Experimental Series I (pp. 305, 306).
    Experimental Series II (pp. 307, 308).
12. *Summary* (p. 309).

1. *Introduction.*

FOR many industrial purposes alternating current of low periodicity is superior both to direct current and to alternating current of high periodicity. The use of such current for illumination under certain conditions produces an obvious perceptual flicker. The object of this investigation was to ascertain what visual effects follow the substitution of a flickering light for a non-flickering one.

The scope of the investigation was considered to be adequately covered by the following scheme:

1. The physiological effects produced on the eyes of persons with normal and abnormal vision.

2. The immediate and the remote effects of a psychological nature following short and long exposures.

---

[1] This work was carried out with the aid of a grant from the Special Committee on the Physiology and Psychology of Vision appointed by the Medical Research Council.

3. The pathological effects produced by prolonged exposures, if any.
4. Under what conditions the apparent flicker can be removed from the light by electrical means and if the removal of the apparent flicker removes at the same time any effects noted under questions 1, 2 or 3 above.

This paper deals with question 1 and part of question 2. The rest is to be dealt with at a future date.

The enquiry into the physiological effects produced by substituting flickering for continuous illumination was dealt with under the following sub-sections:

(*a*) Visual acuity.
(*b*) Stereoscopic vision.
(*c*) Achromatic vision.
(*d*) Chromatic vision. Colour matching and colour naming of large objects.
(*e*) The appreciation of the colours of small objects.
(*f*) Peripheral colour vision.
(*g*) The appreciation of movement.
(*h*) The effect on the pupil reflex.
(*i*) The speed of reading.

2. *The Light Sources employed.*

Practically the whole work was done with two light sources: the half-watt lamp and the naked carbon arc lamp.

The direct current supply was obtained from a private battery of accumulators.

The flickering light was obtained in two ways: (*a*) By interrupting the continuous current by means of a centrifugal mercury break which was driven by an electric motor. Half-watt lamps were the light sources. The rate of flicker could be varied, but corresponded usually to a periodicity of 10 cycles per second. (*b*) By putting an electrically driven sector at the focus of a condenser, the carbon arc lamp being the light source. The rate used in this case could also be varied but corresponded usually to a periodicity of 25 cycles per second. The alternate periods of light and darkness were of equal duration and practically no light left the source during the "dark" periods. The light therefore was at full intensity for 1/100 sec., became darkened for 1/100 sec., and so on. The periodicity was measured by means of a revolution counter and stop watch. Continuous illumination of equal intensity was obtained by placing a grating composed of half clear and half opaque segments in front of the condenser.

In the case of method (*a*) the light did not by any means fall to zero on interrupting the current, because of the heat retained in the filament of the light source. The intensity at different parts of the cycle was therefore measured and the values obtained are shown in Fig. 1. The current was found to be on for about ·035 sec. and off for about ·025 sec. as measured by a capillary electrometer.

Reference to Fig. 1 shows that the intensity varied from about 3·9 foot candles (F.C.) to 7·5 during the cycle, *i.e.* from 52 per cent for the minimum of the "dark" period to 100 per cent. for the maximum point of the bright, when the rate of flicker was approximately 8·5 per second. At slower rates the filament would have longer to cool and therefore a bigger difference would be found to exist.

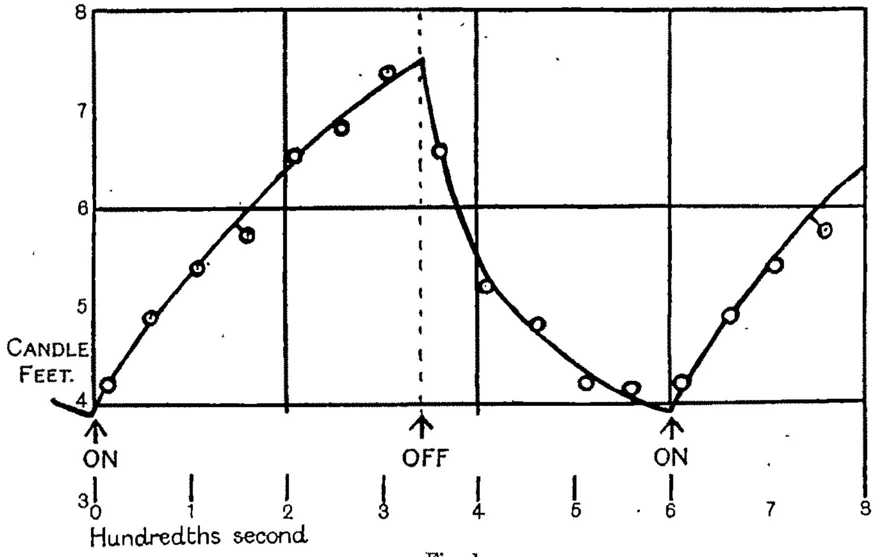


Fig. 1.

3. *The Effects of Flickering Light on Visual Acuity.*

The increasing use of alternating current lighting raises the question: Is such a flickering light as good for visual purposes as a non-flickering source?

So far as acuity is concerned the results appear to be identical, as the following tables show.

TABLE I. *The effect of substituting flickering for continuous illumination on the visual acuity of two normal-sighted observers.*

By Method I.

Rate of flicker: 9 cycles per second (mercury interrupter).

	Flickering, 6 F.C.		Continuous, 6 F.C.	
Visual angle	Number of alphabets	Mistakes per alphabet	Number of alphabets	Mistakes per alphabet
0·86′	18	3·1	6	2·8
0·62′	6	12·5	6	14·0
0·47′	2	17·25	2	16·5

TABLE II. *The effect of substituting flickering for continuous illumination on the visual acuity of normal-sighted observers made artificially defective.*

By Method I.

Illumination 3·5 F.C. Visual angle 0·915′.

	Flickering	Continuous
0·5 D. cy. at right angles	19·0	15·5
1·0 D. cy. at right angles	19·5	20·5

Total number of alphabets = 8.

4. *Stereoscopic Vision.*

It is well known that a large number of factors contribute to give the perception of stereoscopic relief, but that even when all accessory factors are rigidly excluded the normal observer still possesses an extremely delicate distance-recognizing mechanism. This mechanism is not so accurate for instantaneous observation as it is for prolonged observation. A flickering light consisting of a series of very brief exposures might therefore give less acute stereoscopic vision than that obtained with continuous lighting.

The matter was tested with an apparatus by means of which three vertical white threads were exposed to the observer's vision. The two outside threads were 2 metres from the observer and the central thread could be adjusted to the required distance in front of or behind the other two. Only short lengths of the threads were visible. The weights hanging from their lower ends were dipped in viscous oil so as to eliminate vibration and to allow of rapid setting. The lighting was such that no shadows were cast. Using one eye alone the observer had no clue as to the relative position of the central thread to the other two. Parallax was excluded by a head rest.

The central thread was placed 1, 2 or 3 millimetres in front, or at the same distance, or 1, 2 or 3 millimetres behind the other two. The order of the positions was arranged to be irregular by one observer. The

TABLE III. *The stereoscopic acuity of vision of three normal-sighted observers for continuous and flickering illumination.*

Illumination intensity: 3 and 50 F.C. Rate of flicker: 24·2 cycles per second (rotating sector).

O.	S.	No.	Light	+3	+2	+1	0	−1	−2	−3
					NEAR.					
H.	A	30	D.C.	9·3	8·3	3·7	0·7	—	—	—
	B	30	D.C.	10·0	10·0	10·0	2·9	—	—	—
	A	30	FL.	9·3	8·0	6·0	0·7	0·3	—	—
	B	30	FL.	10·0	10·0	10·0	2·9	0·5	—	—
L.	A	60	D.C.	9·8	9·3	8·3	4·3	1·8	0·8	—
	B	60	D.C.	9·8	9·8	9·6	6·7	3·1	1·0	—
	A	60	FL.	10·0	9·3	6·5	3·3	1·0	0·2	—
	B	60	FL.	10·0	9·8	9·7	6·4	1·7	0·2	—
S.	A	20	D.C.	9·5	6·5	6·0	2·0	0·5	—	0·5
	B	20	D.C.	9·5	8·7	10·0	3·3	1·1	—	0·5
	A	20	FL.	9·0	7·5	3·5	4·0	0·5	—	—
	B	20	FL.	10·0	8·9	5·8	5·0	0·5	—	—
ALL	A	110	D.C.	9·6	8·5	6·6	2·9	1·1	0·5	0·1
	B	110	D.C.	9·8	9·7	9·7	5·7	2·0	0·5	0·1
	A	110	FL.	9·6	8·6	5·8	2·7	0·7	0·1	—
	B	110	FL.	10·0	9·7	9·1	5·5	1·1	0·1	—
					EQUAL.					
H.	A	30	D.C.	0·7	1·7	6·3	7·7	5·0	1·0	0·3
	B	30	D.C.	—	—	—	—	—	—	—
	A	30	FL.	0·7	2·0	4·0	7·7	3·7	—	—
	B	30	FL.	—	—	—	—	—	—	—
L.	A	60	D.C.	—	0·5	1·3	3·5	4·2	1·8	0·7
	B	60	D.C.	—	—	—	—	—	—	—
	A	60	FL.	—	0·5	3·3	4·8	4·2	2·3	—
	B	60	FL.	—	—	—	—	—	—	—
S.	A	20	D.C.	—	2·5	4·0	4·0	5·5	0·5	—
	B	20	D.C.	—	—	—	—	—	—	—
	A	20	FL.	1·0	1·5	4·0	2·0	1·0	0·5	—
	B	20	FL.	—	—	—	—	—	—	—
ALL	A	110	D.C.	0·2	1·2	3·2	4·7	4·6	1·4	0·5
	B	110	D.C.	—	—	—	—	—	—	—
	A	110	FL.	0·4	1·1	3·6	5·1	3·5	1·4	—
	B	110	FL.	—	—	—	—	—	—	—
					FAR.					
H.	A	30	D.C.	—	—	—	1·7	5·0	9·0	9·7
	B	30	D.C.	—	—	—	7·1	10·0	10·0	10·0
	A	30	FL.	—	—	—	1·7	6·0	10·0	10·0
	B	30	FL.	—	—	—	7·1	9·5	10·0	10·0
L.	A	60	D.C.	0·2	0·2	0·3	2·2	4·0	7·3	9·3
	B	60	D.C.	0·2	0·2	0·4	3·3	6·9	9·0	10·0
	A	60	FL.	—	0·2	0·2	1·8	4·8	7·5	10·0
	B	60	FL.	—	0·2	0·3	3·6	8·3	9·6	10·0
S.	A	20	D.C.	0·5	1·0	—	4·0	4·0	9·5	9·5
	B	20	D.C.	0·5	1·3	—	6·7	8·9	10·0	9·5
	A	20	FL.	—	1·0	2·5	4·0	8·5	9·5	10·0
	B	20	FL.	—	1·1	4·2	5·0	9·5	10·0	10·0
ALL	A	110	D.C.	0·2	0·3	0·2	2·4	4·3	8·2	9·5
	B	110	D.C.	0·2	0·3	0·3	4·3	8·0	9·5	9·9
	A	110	FL.	—	0·3	0·5	2·2	5·8	8·5	10·0
	B	110	FL.	—	0·3	0·9	4·5	8·9	9·9	10·0

O., Observer; S., Series; A, Observer reports 'near,' 'equal' or 'far'; B, The answers of 'equal' were distributed between the 'nears' and 'fars' in the proportion in which the latter occurred at that sitting; No., Number of observations at each position; D.C. and FL., Continuous or flickering respectively.

other observer, 2 metres away, estimated their relative positions and reported 'near,' 'far' or 'equal' (see later).

The results obtained are given in Table III on the previous page.

The figures + 3, + 2, + 1, 0, etc. show the relative positions of the threads; thus + 2 = middle thread 2 millimetres in front of other threads, and − 3 = middle thread 3 mm. behind other threads. 'Near,' 'equal' and 'far' give the observer's report and the values in the centre of the table give the number of times (out of ten) he gave that report at each position of the threads.

Thus taking the top line the observer H. reported the thread when 3 mm. in front of the others to be 'near' 9·3 times out of 10; to be 'equal' 0·7 times out of 10; and to be 'far' not at all. The comparison between the results obtained with continuous and with flickering light is best obtained by an examination of Figs. 2 and 3.

In Fig. 2 are plotted the number of times the reports 'near,' 'equal' and 'far' were made at the different positions of the threads.

In Fig. 3 the same data are shown but with the answers of 'equals' distributed to the 'near' and 'far' curves in the proportion in which the latter occur at any setting.

It will be seen that there is striking similarity in the curves in both Figs. 2 and 3 for the continuous and flickering light, the efficiencies of stereoscopic vision being approximately the same under the two conditions. A series of short exposures therefore give the same result as one continuous one.

5. *Achromatic Vision.*

The achromatic foveal vision was tested by ascertaining the ability to match two shades of grey. The tests were performed by two normal-sighted observers and two series of tests were performed, one set at approximately 3 F.C. and the other set at 50 F.C.

A Bausch and Lombe colorimeter was employed, the solutions being suspensions of finely divided carbon (Indian ink) in water. The table below, Table IV, gives the span and the mean variation of the readings. The rate of flicker was 24·2 cycles per second obtained by a rotating sector.

TABLE IV.

Light	Intensity	Span	Mean variation
Continuous	High	0·9	0·46
	Low	1·75	0·17
Flickering	High	0·75	0·33
	Low	1·75	0·18

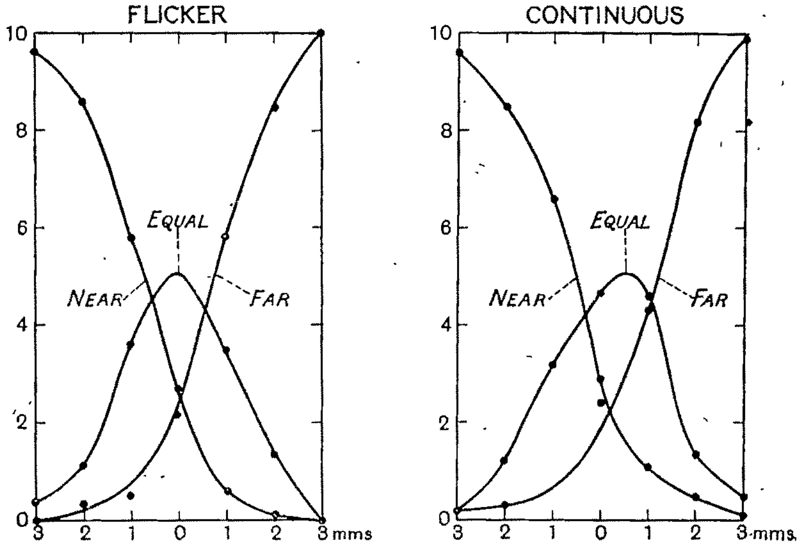

FLICKER
CONTINUOUS
EQUAL
NEAR
FAR
10
8
6
4
2
0
3
2
1
0
1
2
3 mms

Fig. 2.

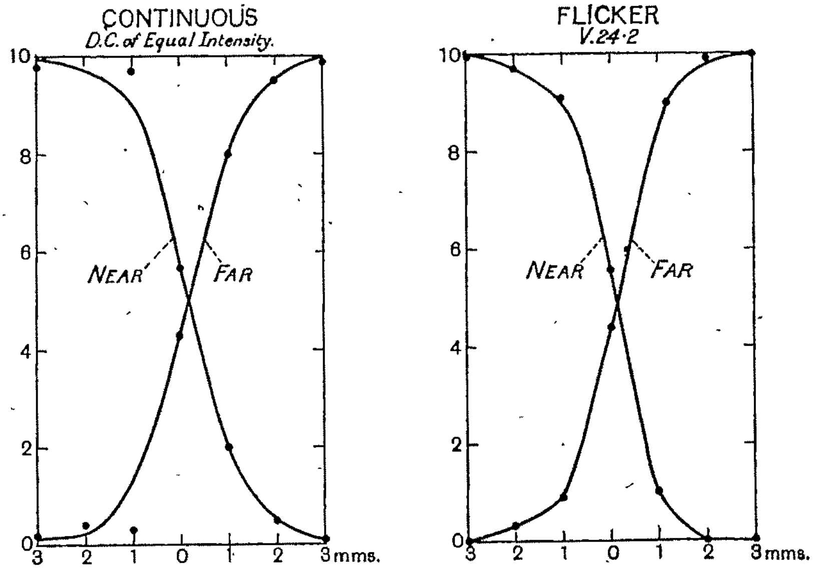

CONTINUOUS
D.C. of Equal Intensity.
FLICKER
V.24·2
NEAR
FAR
10
8
6
4
2
0
3
2
1
0
1
2
3 mms.

Fig. 3.

The results at low intensity are nearly identical at the high intensity; those obtained with the flickering light are somewhat better with the low. This result was probably due to the difficulty of arranging a perfectly even illumination of the colorimeter with the continuous illumination. The number of observations with each intensity was 48.

6. *Chromatic Vision.*

(*a*) *Colour Matching.* The same technique was used as for (*c*) above, except that coloured fluids were used instead of neutral tint ones.

The results obtained are summarized in the following Table V. It will be noticed that there is close similarity between the values obtained with the flickering and continuous light.

TABLE V. *The colour matching ability of two normal-sighted observers.*

Rate of flicker: 24·2 cycles per second (rotating sector).

			Continuous		Flickering	
Colour	Series	Number of observations with each type of illumination	Span	Mean variation	Span	Mean variation
Red	1	24	5·1	0·38	5·3	0·40
	2	48	2·0	0·24	2·2	0·19
Blue	1	24	3·5	0·28	3·0	0·37
	2	94	1·7	0·19	1·75	0·14
Yellow	1	24	4·9	0·51	4·8	0·52
	2	48	3·4	0·46	3·9	0·47
Mean	1	—	3·6	0·32	3·5	0·35
	2	—	1·9	0·32	2·1	0·26

Series 1. Low intensity (3 F.C.). Series 2. High intensity (50 F.C.).

(*b*) *Colour Naming.* From 121 skeins of wool, 41 were selected in bright day-light as forming eight series in which very close similarity of tint occurred. In the following table the general colour of each of the series is given, also the number of skeins in each series, and the number of mistakes made in describing the differences in tint between the various skeins. The descriptions were stated in writing and were corrected by making the comparison between the answer made by the artificial light and by day-light.

It will be seen that with the exception of series 2 (blue-green) and series 6 (pale brown) there is close similarity between flickering and continuous light. In these two series the results by flickering light were somewhat better.

TABLE VI. *The ability of a normal-sighted observer to name the colours of series of skeins of wool in continuous and flickering light.*

Illumination: 8 F.C. Rate of flicker: approx. 25 cycles per second (rotating sector).

Series	No. of wools in series	Number of mistakes made in 4 exposures of each wool	
		Continuous	Flickering
1 Mauve	4	0	0
2 Blue-green	3	1½	½
3 Green	5	½	1
4 Pink	6	4	3½
5 Dark brown	7	½	0
6 Pale brown	6	2	½
7 Yellow	5	1	1
8 Slate	5	½	1½
Total mistakes	—	10	8

TABLE VII. *Colour naming of two normal-sighted observers, for coloured threads on black or white backgrounds.*

Illumination: 4 F.C. Rate of flicker: 9 cycles per second (mercury interrupter).

Black background	Flicker				Continuous				Frequency of mistakes
	No. of times shown		Mistakes		No. of times shown		Mistakes		
Colour shown	H.	L.	H.	L.	H.	L.	H.	L.	Both
Red	4	4	0	0	5	4	0	0	0·0
Orange	3	2	1	1	1	3	0	0	0·2
Yellow	4	4	0	0	4	4	0	0	0·0
Sage-green	4	4	1	0	4	4	3	1	0·3
Emerald-green	4	4	0	0	4	4	0	0	0·0
Dark blue-green	4	4	4	4	4	4	1	3	0·75
Blue	2	2	2	1	2	2	1	0	0·5
Light mauve	4	5	2	0	3	3	3	1	0·4
Dark mauve	2	2	0	0	1	3	1	1	0·25
Pale pink	4	4	3	1	4	4	4	3	0·7
Pale blue	4	4	4	1	4	4	3	0	0·5
Russet	4	4	0	2	3	4	0	4	0·4
White	4	4	0	1	4	4	1	3	0·3
Grey	0	1	0	0	1	1	1	1	0·7
Black	4	4	1	1	4	4	2	0	0·25
Totals	—	—	18	12	—	—	20	17	—
White background									
Red	5	6	0	0	5	6	0	0	0·0
Orange	3	3	0	0	2	3	1	1	0·2
Yellow	4	4	1	0	4	4	1	1	0·2
Sage-green	3	1	2	0	3	2	2	0	0·4
Emerald-green	4	4	0	0	4	4	1	0	0·1
Dark blue-green	4	4	4	4	4	4	2	0	0·6
Blue	5	6	4	4	5	6	1	4	0·6
Light mauve	0	2	0	1	1	1	1	0	0·5
Dark mauve	4	2	1	0	3	2	3	0	0·4
Pale pink	4	4	4	0	4	4	1	0	0·3
Pale blue	4	4	1	1	4	4	1	0	0·2
Russet	3	2	3	1	3	3	2	3	0·8
White	4	4	0	0	4	4	3	1	0·25
Grey	4	4	4	4	4	4	4	4	1·0
Black	2	0	1	0	2	1	0	0	0·2
Totals	—	—	25	15	—	—	23	14	—

Total mistakes: Flickering 70; Continuous 74.

7. *The Appreciation of the Colours of Small Objects.*

Coloured threads (cotton or silk) of approximately the same diameter were mounted in lines across either black or white cardboard. The cards were viewed at a distance of about 2 metres; the intensity of the flickering and continuous illumination were both approximately 4 F.C. The results are shown in Table VII.

An inspection of Table VII shows that there are differences in the numbers of mistakes made in the individual colours under the two different methods of illumination. The total mistakes are, on the other hand, in very close agreement, viz. 70 for the flickering and 74 for the continuous lighting.

8. *Peripheral Colour Vision.*

An ordinary perimeter was used for testing the colour vision of the periphery, and the standard coloured papers were used as test objects. While theoretically there is no doubt that spectral colours would have been more in accordance with the most recent methods of research, this did not justify the expense of supplying the necessary elaborate apparatus, since coloured papers would afford the comparison which was required between the two methods of lighting. The results obtained are shown in the table below.

TABLE VIII. *The peripheral colour vision of two normal-sighted observers by continuous and flickering illumination.*

Illumination intensity: 6 F.C. Rate of flicker: 9 cycles per second (mercury interrupter).

Colour	Observer	Light	Apparent colours before correct answer was given	Average angle from line of vision at which colour was correctly named. Degrees
Blue	H. H.	D.C.	Brownish, Pale blue	60
		FL.	Bluish green, Purple	64
	R. L.	D.C.	None	77
		FL.	None	76
Red	H. H.	D.C.	Brownish	46
		FL.	Yellow, Orange	47
	R. L.	D.C.	—	—
		FL.	Blue, Orange	32
Yellow	H. H.	D.C.	Yellowish, White, Apple-green	14
	R. L.	FL.	Yellowish, Greenish, White	17
	R. L.	D.C.	Whitish, Greenish	10
		FL.	Whitish, Yellowish, Greenish	10
Green	H. H.	D.C.	Whitish, Apple-green	18
		FL.	Yellowish, Greenish	21
	R. L.	D.C.	Whitish, Greenish	12
		FL.	Whitish, Greenish, Yellow	12
White	H. H.	D.C.	Whitish, Yellowish, Apple-green	15
		FL.	Whitish, Yellowish, Apple-green	12
	R. L.	D.C.	Bluish or Greenish	17
		FL.	White, Greenish	20

An inspection of the table shows that on the whole there is no marked difference between the results obtained by the two different methods of lighting.

9. *The Perception of Movement.*

This perception was measured by presenting to the eye of the observer an aperture behind which moved a ribbon of white paper to which was attached black strips of paper. The number of possible variables is very great.

(*a*) Intensity of light illuminating to test object.

(*b*) Angle subtended at the eye between point of fixation and exposure aperture.

(*c*) Angular rate of movement of the strips.

(*d*) Angular size of aperture vertical and horizontal.

(*e*) Angular size of strips.

(*f*) Ratio of intensities of black strips to white background.

(*g*) Intensity of light illuminating surroundings of test object and in particular the fovea.

Tentative experiments showed that each of these factors is of importance; these tests when completed are to be published elsewhere.

But keeping all factors constant except intensity we investigated the relationship between rate and the greatest angle at which it can be observed for both flickering and non-flickering light.

Our results are shown in the table below for the observer R. J. L., the periodicity being approximately 10 cycles per second at 5·0 and 0·035 F.C.'s and 25 cycles at 0·6 F.C.'s.

TABLE IX.

Breadth of black strip subtended 1·25 degrees at eye. Opening 5 × 2·5 degrees.

Speed in degrees per sec.	Intensities					
	5 F.C.		0·6		0·035	
	Fl.	Cont.	Fl.	Cont.	Fl.	Cont.
0·52	69	87	90	94	80	83
2·6	92	101	100	103	97	96
5·2	95	104	105	106	98	100
26·0	98	107	104	105	95	100
52·0	97	105	97	98	81	83

An inspection of the above table shows that at the slow speeds and the high intensity there is an obvious difference between the results given by the continuous and flickering lights. The continuous light permits movement to be seen with more peripheral vision than does the other. As the intensity is decreased or the rate increased, or both, the difference became less. The same differences were found in the case of the second observer (H. H.).

This point is important because the negotiation of traffic is largely dependent on peripheral vision. The lighting of streets by low period and obvious flickering lighting might lead to accidents.

To test this point further the lamps were therefore run at a periodicity of approximately 25 cycles per second. It was now found that the differences between the flickering and continuous light became inappreciable.

10. *The Effects on the Pupil Reflex.*

There seems to be very strong evidence that the function of the pupil reflex is to protect the retina from sudden changes in illumination. It might be supposed therefore that a flickering light would bring this mechanism into play to excess and that fatigue would result. Some tests were therefore performed to ascertain the activity of this reflex before and after several hours' exposure to a flickering light.

The activity was measured in the following manner.

There is a well-known pathological condition in which the pupil rhythmically increases and decreases in diameter which is known as Hippus. Now an artificial hippus can be produced in a normal-sighted observer by looking at a bright light source with one eye while at the same time a small black metal disc of 5 mm. diameter is supported on a wire immediately in front of the cornea so as to obstruct the pupillary opening. Since rays to the retina are now largely excluded, the pupil dilates so that it becomes larger than 5 mm. Light rays now enter the eye, and the pupil in consequence constricts, thus obstructing the light again. Then the whole process is repeated. As a result the observer sees the light for a short time and then it disappears to reappear again a few moments later. The observer can therefore himself record the rate of the artificial hippus either by counting the number of glimpses that he has received of the light source in a given time period or better by recording on a steadily moving smoked surface the opening and closing of the pupil by making corresponding movements of a recording lever with the hand.

The reason for the second method being better is that periodically the pupil reflex stops acting for a short period and then picks up the rhythm again in the same phase as if no interruption had occurred. On the record it is a simple matter to insert the lost contractions which are usually single and never more than two in number.

The following table shows the periodicity of the contractions in the tests made in the above manner.

TABLE X.

	1st Test		2nd Test (5 hrs. after 1st Test)	
Observer	Periodicity. Average time	Periodicity. Lost beats inserted	Periodicity. Average time	Periodicity. Lost beats inserted
H. H.	1·07	1·20	1·175	1·27
R. J. L.	0·69	0·9	0·87	1·07

A comparison of the two sets of tests shows that the periodicity was rather quicker after the five hours' exposure to the flickering light than it was before. For example where, as in the case of the observer H. H., 1·07 contractions occurred per second before the exposure, 1·20 contractions occurred after it. This would seem to indicate that since the pupil reflex is brisker it cannot be suffering fatigue. Possibly this is explained by the big difference between the periodicity of the light source and that of the pupil reflex. The latter is, as it were, tuned to roughly one contraction per second and will not respond to a higher rate of stimulation.

11. *The Speed of Reading*[1].

Experimental Series I.

(*a*) *The Subjects.* Four subjects were employed in these experiments; they had acted as subjects in similar experiments on legibility of type during the past year.

Their visual acuity was as follows:

Subject A.
$RV = \frac{6}{5}$ (3 errors)
$LV = \frac{6}{5}$ (3 errors) Minute degree of astigmatism.

Subject B.
$RV = \frac{6}{5}$
$LV = \frac{6}{5}$ Very low degree of hypermetropia.

Subject C.
$RV = \frac{6}{5}$ (1 error)
$LV = \frac{6}{5}$ (3 errors) No refractive error.

Subject D.
$RV = \frac{6}{6}$ (2 errors)
$LV = \frac{6}{6}$ Slight myopia.

---

[1] This section of the work was carried out at the Cambridge Psychological Laboratory by Miss W. V. Matthews under the general direction of Mr F. C. Bartlett. The whole of the rest of the work was carried out by Dr Hartridge and Mr Lythgoe in the Cambridge Physiological Laboratory.

None of the subjects wore spectacles during the experiments, they had been selected as having comparatively normal vision. In series 1 given on seven consecutive days two subjects (A. and B.) came alternatively at 10 A.M. or at 2 P.M. and two (C. and D.) after their day's work, at 5 or 6 P.M.

Series 2 was given at intervals throughout a day spent entirely in the flickering light; it was followed on the next day by an evening session in continuous light.

(*b*) *Typographical Material.* Pages of ordinary English (Burke's Speech on American Taxation) set up in an ordinary type (Lanston Monotype Old Face Imprint 10 point) were used. All paragraphs, italics, figures and reiterations had been eliminated. The pages were marked so that the subject started at a different position on the page at each exposure.

(*c*) *Apparatus.* The pages to be read were exposed in a tachistoscope, pointers indicating a mark at which the reading was to start. Each exposure lasted 4 seconds and was followed by an interval of 11 seconds, or more if the material were not ready. During the interval the eyes of the subject rested on the green shutter of the tachistoscope.

(*d*) *Illumination.* The light was cast by a mirror from an arc lamp and illuminated the area of the tachistoscope. The intensity of the light was kept as nearly as possible at 5 foot candles, but it was not possible entirely to eliminate a slight coloration.

The rate of flicker was equivalent to 24·2 cycles per second obtained by a rotating sector; the latter made a considerable amount of noise.

(*e*) *Procedure.* Sessions of one hour were given on seven consecutive days. The subject sat at the table on which the tachistoscope was placed and at normal reading distance from it. On entering the room the subject read to himself while he grew accustomed to the light.

On the first four days the procedure was as follows:

5 minutes' reading as the subject chose in continuous light.
20 minutes occupied by 40 exposures in continuous light.
15 minutes' reading as the subject chose in flickering light.
20 minutes occupied by 40 exposures in flickering light.

In the test experiments the subject read aloud from the mark on the page before him, the last word read and any mistakes were recorded. The scores show the number of letters correctly read.

(*f*) *Results.* Number of letters read in *flickering light* expressed as a percentage of number read in *continuous light* on the same day by each subject.

TABLE XI.

	Subjects			
	A.	B.	C.	D.
Days 1	96	92·0	107	94·0
2	103	92·0	97	102·0
3	97	96·0	107	98·0
4	94	94·0	105	101·75
5	102	89·9	95	106·0
6	99	89·0	95	99·0
7	92	95·5	102	101·0
Average	98	93·5	101	99·0

Experimental Series II.

(*a*) *Illumination.* Both continuous and flickering light were from above and illuminated the whole room. There was no noise.

(*b*) *Procedure.* Sessions of 40 mins. were given at intervals of 80 mins. throughout the day which was spent entirely in the flickering light. Three of the same subjects were employed, A., C. and D.; B. was unable to take part. Method and apparatus were exactly as before. Each session consisted of 80 exposures. A similar session was given on the following day in continuous light. After 20 mins.' silent reading to accustom the subject to the light, the first session of 80 exposures was given. Then the subject read to himself as he pleased, sketched, etc., etc., for 80 mins. while the others were each given 40 mins. This was repeated five times during the day, so that each subject spent 9 hrs. in the flickering light.

(*c*) *Results.* Number of letters read at each session in *flickering light* expressed as a percentage of number read in *continuous light* on the next day (session 6).

TABLE XII.

	Subjects		
	A.	C.	D.
Session 1	99	105	96
2	96	100	100
3	98	98	100
4	99	95	96
5	101	96	94
Average	98	98	97

Subject A., after an initial high score dropped, and then improved throughout the day till her last score in flickering light is above that in continuous light on the next day.

Subject C. read most at his first session and his score diminishes at each succeeding session.

Subject D. improved until his third session when his score drops so that the number he read in continuous light the next day was 2 per cent. above his average for the day spent in flickering light.

(*d*) *Introspections of subjects.* A., C. and D. all found the flickering light 'disagreeable' at first but soon ceased to notice it. They reported no ill after-effects; they were 'tired' at midday but thought they were reading well in their later sessions.

(*e*) *Results of Experimental Series I and II.* The scores of the subjects are higher in the second series than in the first. This may be due to the disturbing sound during Experimental Series I. A month had elapsed between the two series during which the subjects had been for their holidays.

On the whole very slightly fewer letters were read in flickering than in continuous light in both cases, but considering the great fluctuations in scores in one light only this small difference cannot be considered significant.

(*f*) *Results of Subject* B. The subject B., who had served in Experimental Series I (sessions in both lights on one day for seven consecutive days), was unable to attend Experimental Series II and was subsequently given a similar series.

He worked for seven sessions in flickering light on one day under exactly similar conditions to those under which the other subjects had worked. On the following day he was given a similar series in continuous light.

In Experimental Series I his scores in continuous light were consistently higher than those in flickering light. This is again the case when sessions were given throughout two consecutive days.

Results.

Number of letters read at each session in *flickering* light expressed as a percentage of the number read in *continuous* light in the corresponding session on the following day:

TABLE XIII.

Session No.	1	2	3	4	5	6	7	Average
	92	94	88	99	90	95	96	93·4

NOTE. When in Series I sessions were given on seven consecutive days in both lights the number of letters read in flickering light expressed as a percentage of the number read in continuous light was:

Session No.	1	2	3	4	5	6	7	Average
	90	94	94	96	90	94	98	93·7

In general, therefore, either the experiments failed to measure the effect on the speed of reading of substituting a flickering for a continuous light, or the effect was very slight indeed, viz.: an almost negligible decrease with the flickering light.

In the case of one subject however the flickering light appeared definitely disturbing. This "individual difference" obviously calls for further investigation.

12. *Summary.*

A comparison was made between the continuous and flicker lights for the following visual tests:

(1) The perception of the shape of small black and white objects.

(2) Stereoscopic vision.

(3) The least perceptible intensity difference in the case of large objects.

(4) The colour perception of both large and small objects.

(5) Peripheral colour vision.

(6) The appreciation of movement by the periphery of the retina.

(7) The pupil reflex.

(8) The speed of reading.

With the exception of (6) and (11) (Subject B.) the results obtained are practically identical for the two types of illumination. In the case of (6) the appreciation of movement diminished in flickering light. The difference practically disappears when 25 cycle alternating current is used with medium candle power half-watt lamps.

(9) It remains possible that a more extensive study of individual differences would show more pronounced effects of flicker in certain cases.

(*Manuscript received* 16 *November*, 1925.)

CHAIN INSTINCTS IN LAMBING SHEEP

By A. H. H. FRASER.

(*Kilgour Scholar in Natural Science, University of Aberdeen.*)

The following observations are compiled from notes made while working as a lambing shepherd during the season of 1923. The sheep were of three different breeds, Oxford, Half-bred, and Cheviot. The farm was arable low-ground, situated in Berwickshire.

The behaviour of sheep at lambing time is almost purely instinctive. It is necessary to define precisely what we mean by an instinct, as the term is often loosely employed. This is difficult to do without long discussion. We agree with Professor J. Arthur Thomson that "The routine of instinctive behaviour has often a considerable degree of perfection the very first time, and while it may be improved by practice, it certainly does not require learning or experimenting. In other words, the instinctive behaviour depends upon a hereditary predisposition of the nervous system," further, "That instinctive behaviour is always adaptive to the normal conditions of the animal's life, though it may prove ineffective or misleading in face of peculiar exigencies[1]."

The behaviour of lambing sheep, as is shown below, fits in perfectly with this description. We may therefore term it instinctive, thus emphasising the fact that it is not intelligently 'learned.'

By chain instincts we mean that succeeding phases of instinctive behaviour follow one another in a constant and definite order. That is all we observe objectively, but we think we are justified in assuming that the completion of one phase of instinctive behaviour acts as a stimulus for the appearance of the next.

The first indication that a ewe is about to lamb is that she leaves the flock and wanders restlessly about the field. As a sheep also leaves the flock when ill, one might assume that it was a sensation of sickness that caused the ewe to seek isolation. But the appearance of the sheep in the two cases is entirely different, so that a shepherd can tell at a glance whether a ewe is ill or about to lamb. The sickly sheep may wander to shade, or water, and is often to be found lying at the hedge-side. The lambing sheep goes to gates, to low places in the hedge. To put it subjectively, the behaviour of the lambing sheep is much more purposeful. It is not impossible that some intelligent or inferential processs is at work as well as the instinctive prompting. When watching a ewe wandering restlessly backwards and forwards along a hedge-side, one often wonders where she would go were no obstacle in the way. In some cases in our experience a ewe has crept through a hedge or even jumped a gate and lambed several fields away from the flock. It is quite evident that the commencement of labour arouses an instinctive 'desire' for isolation in the sheep. We may term this the *isolation instinct.*

---

[1] *The System of Animate Nature*, I, 200.

Once the insistent demand for isolation has passed away, the ewe searches out a place to make a bed. Using her fore-feet alternately she scrapes a nest, seldom leaving it if undisturbed until the lamb is born. On a level grass field this action appears quite useless, but it is possibly an instinctive action which was of significance to the sheep in its wild condition, or even in domestication under hill farming conditions. A similar interpretation is usually given to the action of the dog in turning round and round on the hearth before lying down. This emphasises the hereditary nature of instinctive behaviour, since an instinct may persist after all its significance has disappeared.

Even before labour has actually commenced, the maternal instinct is aroused. This is proved by the fact that the ewe is attracted by any lamb in her vicinity. Sight and hearing act as stimuli, probably smell may also act. That sight plays a part in exciting the maternal instinct is shown by the fact that a bundle of white rags may sometimes be used successfully as a decoy to a lambing sheep. That she is also excited by hearing is proved by the fact that she may run towards the sound of a lamb's bleating, when the lamb is out of sight and down-wind.

The passage of the bag of membranes through the maternal passages appears to intensify the maternal instinct greatly, and after the rupture of the membranes great difficulty is often experienced in preventing what we may call 'lamb-theft,' that is the lambing ewe attempting to mother the lamb of another sheep. It is no unusual occurrence after a brief absence from the lambing-field to find two ewes licking the same lamb and even both allowing it to suck. In such cases the intruding ewe is very often the more attentive to the lamb. Once a lambing ewe has licked another lamb, it is often some time before her pains recommence, and this delay in parturition may result in the death of her own lamb. This is a striking example of a perversion of instinct, but were the earlier isolation instinct satisfied, it would seldom if ever occur. Lamb-theft, although frequent, varies greatly in intensity and is seldom marked in gimmers (primiparous sheep). This may be associated with the fact that the maternal instinct is not so well developed in gimmers as in ewes, as is shown by the much greater frequency of lamb desertions in the former.

After the membranes have burst, the ewe licks up the spilt amniotic fluid, and is almost as unwilling to leave the place where it has spilt as she afterwards is to leave her lamb.

Numerous observations led us to the conclusion that the stimulus for the full awakening of the maternal instinct in sheep is the taste of the amniotic fluid. The evidence although not final, is at least suggestive. Lamb-theft serves as a useful test for the strength of the maternal instinct. The evidence may be summarised as follows:

1. Lamb-theft is rare in ewes before the membranes have burst.

2. It is frequent in sheep which have licked up their own amniotic fluid.

3. If a lambing sheep approaches the lamb of another sheep, it may be successfully driven away.

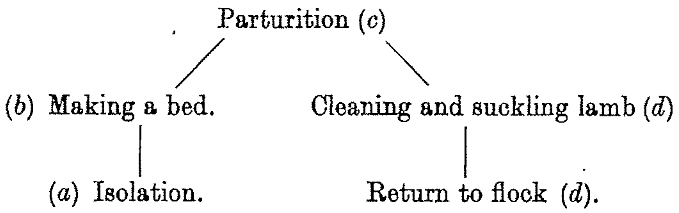
4. If, however, it has licked the lamb, it can never be driven away, it must be separated from it. Even then it may actually jump a fence to return.

5. If a sheep (usually a gimmer) has deserted its lamb, it will take it again provided it can be induced to lick it.

6. Further, once a sheep has licked its lamb, it will never desert it.

How are we to interpret these various phases of behaviour in the lambing sheep, on the one hand apparently perfectly adapted to the preservation of the species, on the other hand so readily disturbed? In particular, how are we to explain lamb-theft, which may result in the death of the unborn lamb? Is it the result of degeneracy due to domestication, so often assumed when things go wrong among domestic animals; or is it perhaps due to disturbance of behaviour due to unnatural environmental conditions? We believe the latter to be the case. We think that the instinctive behaviour of the lambing sheep may be analysed into several phases, each phase acting as the stimulus for the development of the next, and that this succession of phases is perfectly adapted to the preservation of the species. In fact we have to do with a chain of instincts.

We may represent this chain in diagrammatic fashion.



Disturbance of any link of this chain will lead to the disorganization of the whole train of behaviour. Thus in lamb-theft the stimuli due to the presence of a lamb, stimuli of sight, hearing, and probably smell which would normally occur between (c) and (d), owing to artificial obstacles to the isolation instinct occur somewhere between (a) and (c). We thus see that lamb-theft is due to artificial environmental conditions which prevent isolation. Stimuli which should normally operate only after parturition may act before it with the results described. Lamb-theft may thus be regarded as a perverted instinct. There, as elsewhere, perversion of instinct follows prevention of its normal course.

GENERALIZATIONS AND CONCLUSIONS.

From a study of the behaviour of lambing sheep we may draw the following generalizations and conclusions.

1. The most striking feature of a lambing sheep's behaviour is its individuality. No two sheep behave exactly alike and anything but general prediction is impossible.

2. The fundamental basis of its behaviour is instinctive, but every instinct and particularly the maternal is improved and perfected by experience. This is very noticeable in the different behaviour of ewes and gimmers. The evident alarm of a gimmer at the appearance of its own lamb is often very comical, but a ewe quite evidently knows what she is about. Further, an old three or four crop ewe is usually the best mother.

3. The normal chain of instincts in the lambing sheep is perfectly adapted to natural environmental conditions.

4. Under arable farming conditions this chain of instincts is only accidentally satisfied. The result is often lamb-theft, which can be regarded only as a perverted instinct.

(*Manuscript received* 20 *December* 1925.)

GROUP TESTS IN REASONING ABILITY

By A. BARBARA DALE.

1. *Introductory* (pp. 315, 316).
2. *Construction of the test* (pp. 316–318).
    - *A. Subject matter* (p. 317).
    - *B. The time factor* (p. 317).
    - *C. The 'guess' factor* (pp. 317, 318).
    - *D. Recording the solution* (p. 318).
    - *E. Scoring the test* (p. 318).
3. *Preliminary trials* (pp. 318–321).
    - *Relative difficulty of the problems* (p. 319).
    - *Diagnostic value of each problem* (pp. 320, 321).
4. *Administration of the test* (pp. 321–325).
5. *Results of testing* (pp. 326–337).
    - (*a*) *Children tested* (pp. 326, 327).
    - (*b*) *Consideration of the scores* (pp. 327, 328).
    - (*c*) *Reliability of the test* (p. 328).
    - (*d*) *Comparison with other measures of ability* (pp. 328, 329).
    - (*e*) *Effect of age* (pp. 329–331).
    - (*f*) *Age limits* (p. 331).
    - (*g*) *Effect of sex* (p. 331).
    - (*h*) *Comparison of the group with the individual test* (pp. 331, 332).
    - (*i*) *Time factor* (p. 332, 333).
        1. *Relation between score and speed of working* (p. 333).
        2. *Relation between speed and accuracy* (pp. 333–335).
        3. *Speed and improvability* (p. 335).
        4. *Diagnosis by comparison of results in tests of limited and unlimited time* (pp. 335–337).
6. *Conclusions* (pp. 337, 338).

1. *Introductory.*

THE object of this investigation was to explore the possibilities of co structing from a series of problems, such as those constituting Burt Reasoning Test, a reliable group test of reasoning ability that coul be administered, scored and interpreted without expert knowledge training.

Since the power to reason is of the utmost social importance, th school must be concerned both with its existence in children and wit the possibility of helping its development in each individual. Cons quently it becomes the task of the educational psychologist to inve means for its measurement. Moreover, reasoning ability appears closel bound up with general intelligence. Our conception of reasoning abilit may involve a factor such as the 'sagacity' of William James, or w may adopt Binet's definition of reasoning power as "the power of appe ception and association which makes possible the transference from known fact to a second unknown fact by means of a resemblance." I either case we can detect little difference between our definition reasoning and such accepted definitions of general intelligence as "th power to carry on abstract thinking[1]," or "the general capacit of an individual consciously to adjust his thinking to new requir ments[2]."

On the practical side, experiment has shown that intelligence exhibited most characteristically in dealing with new situations, an in performances requiring constructive thought, and can therefore b measured best, as Burt affirms, by tests "involving higher mental pr cesses, particularly those involving reasoning[3]." This has been co firmed by the writer in a study of the scores registered in the differe sections of the National Intelligence Test[4].

Isolated attempts to measure children's ability to reason can be foun as far back as 1896, but the only tests widely used to-day are Burt individual test and Winch's group of simpler logical problems. Both i the hands of the expert have yielded very valuable results, but the ti and experience necessary for the administration of the one and for th subsequent diagnosis of the results of the other prohibit their wide use in schools. As far as the writer is aware, there was not in existence, at the time when this investigation was begun, any other group reasoning test that could be used easily and with a fair prospect of reliability by the ordinary school teacher.

---

[1] L. M. Terman, "Intelligence and its Measurement," *Journ. Educational Psycholog* XII, 1921.

[2] W. Stern, *Psychological Methods of Testing Intelligence*, p. 88.

[3] C. Burt, "The Development of Reasoning Ability in School Children," *Jour Experimental Pedagogy*, V, 1919.

[4] J. C. Chapman and A. B. Dale, "A Further Criterion for the Selection of Ment Tests," *Journ. Educational Psychology*, XIII, 1922.

2. *Construction of the test.*

Much of the material used was borrowed from Burt's test[1], since here was material carefully selected and proved to be of practical usefulness.

The plan of construction for the test may be summarized briefly:

A. The test is to measure objectively reasoning power in children, as exhibited, not in mathematical or in formal logical work, but in the solution of straightforward problems drawn from everyday life.

This necessitates:

(1) A complete understanding of the material by the child; the subject matter must be connected with his own concerns, and the language and form of statement readily comprehensible.

(2) An avoidance of the subject matter of the schoolroom, the effects of training being still further eliminated by making no demand for facile use of language, or any but the simplest arithmetical work.

(3) A lively interest in the subject matter, with a solution of practical importance in the child's eyes, to insure maximum attention and persistence.

(4) A sufficiency of data, so that nothing relevant is omitted, and yet nothing irrelevant included.

(5) The possibility of solving each problem by simple induction or deduction, without requiring judgment derived from past experience other than that common to all children of the ages dealt with. This debars all 'catch' questions.

B. Rapidity of mental functioning is not to be included as an integral part of the measurement unless and until experimental evidence indicates a definite advantage to be gained thereby. In the meantime each child will be allowed, as far as possible, as long as he likes to finish the work.

---

[1] The author wishes to acknowledge her great indebtedness to Professor Burt for his generosity and help, without which her work would have been impossible.

C. The possibility of scoring by guess-work must be made as low as possible.

D. The recording of solutions must be made as simple and brief as possible.

E. The marking must be absolutely objective and free from ambiguity.

A. Subject matter. The subject matter in Burt's questions was obviously suitable as regards familiarity, interest, type of problem and language, although in some cases the form was unsuitable for a group test. Three problems were adopted unchanged, about thirty more with changes in form or wording necessitated by the changed test conditions, and fourteen problems—either entirely new or 'chestnuts' *réchauffés*—were added. The language throughout was revised in the light of Thorndike's *Teachers' Word Book.* With a few exceptions, such as proper nouns, all the words were among the 5000 words there quoted as most commonly used by children, and in no case did the solution of a problem depend upon an intimate knowledge of an unfamiliar word. By a series of preliminary trials possible points of ambiguity and undesirable difficulties were gradually eliminated, and finally there seemed to be no case in which sound reasoning would not lead to one invariable solution.

Problems calling for simple arithmetical processes and word or sentence completion gave fairly satisfactory results, although in the first instance they were included with some hesitation. Inventive power was not greatly demanded; the majority of problems involved deduction only, and comparatively few needed the creation of a hypothesis.

The intrinsic interest of the test, which is especially vital in a long test, was found to be quite adequate; it was not so important, perhaps, in the case of low scorers, but for the most part there was eagerness to begin, good application throughout, and often a spontaneous demand for a further instalment.

B. The time factor. This will be discussed at a later stage in the light of the results obtained; the initial determination was to measure ability by the difficulty of the work that could be done, rather than by the speed of the performance.

C. The 'guess' factor. The impossibility of individual cross-questioning in a group test made it necessary to demand the solution in a form which would very improbably be reached by guess-work alone. For this reason several problems were put in such a form as:

"It looks like rain but I shall not go out to-day.

Draw circles round the numbers of the two true sentences among the following:

1. It will not rain to-day.
2. I shall get wet to-day.
3. I shall stay indoors to-day.
4. I shall not need an umbrella to-day.
5. To-day nobody will need an umbrella."

The original form of Burt's problem was: "It looks like rain but I shall stay indoors. Shall I want an umbrella?"—a problem answerable on paper by 'Yes' or 'No,' with a 50 per cent. chance of guessing right. In its new form the probability of a right answer by guessing is reduced to 1 in 10, and in no problem was the probability allowed to rise above this value.

D. Recording the solution. The methods used were three:

(*a*) the marking of certain sentences, as illustrated above,
(*b*) underlining or crossing out certain words,
(*c*) the writing of a very few words.

E. Scoring the test. The only possible objective method was to give one mark for an answer entirely right, and no half credits for partially right answers. Each problem was so expressed that an answer only partially right—*e.g.* only one sentence marked—made it very doubtful if the child had really been able to grasp and tackle the problem successfully.

3. *Preliminary trials.*

After some individual testing to eliminate outstanding shortcomings in the text, the problems were given as group tests to about 300 children of ages from 10 to 16 years in three Elementary Schools in or near New Haven, Conn., U.S.A.[1] The problems were given in varying order, to eliminate any general practice effect; fifteen or sixteen were worked at one sitting, instructions and practice with three similar problems being given before the first trial and if necessary before the following trials.

The data so obtained were used for several purposes: to show where shortcomings still existed in the test material, to give rough average scores for the different grades, and especially to determine the relative difficulty and diagnostic value of each problem.

[1] The greater part of this work was done at Yale University, U.S.A., and the writer would like here to acknowledge her indebtedness to the late Dr J. Crosby Chapman, Professor of Educational Psychology, under whose guidance the work was carried out, and to whom in great measure is due whatever value it may have.

Relative difficulty of the problems. A general qualitative estimate of the relative difficulty of the problems was found directly from the percentage of children solving each problem correctly. With a view to dividing up the problems into two or three parallel forms of test, however, it was felt that a quantitative measure of difficulty was required, and, owing to the attenuation in the number of cases at each end of the normal distribution curve of scores (the distributions were sufficiently normal to be treated as belonging to that type), it could not be inferred that the increase in difficulty from a problem solved by 55 per cent. of the group to one solved by 50 per cent. was equal to the increase from a 75 per cent. to a 70 per cent. problem. The percentage of solutions was therefore transmuted into terms of σ, the variability of a perfectly normal distribution. This gave the difficulty values of the problems as their deviations, in terms of σ, from a problem solved by exactly 50 per cent. of the group; the values ranged from $-0{\cdot}92\,\sigma$ to $+2{\cdot}67\,\sigma$, these extreme problems being solved by 82 and 1 per cent. of the whole group respectively. Between these the ground was fairly evenly covered, and by an inspection of the σ values of difficulty it was found possible to divide the whole group up into three parallel series or forms, each having about equal steps of difficulty between successive problems, and the same total σ value (see Table I).

TABLE I. *Showing for each problem in the three forms of the test the percentage of right solutions, and hence its difficulty in terms of* σ, *and* C, *the coefficient of internal consistency*—i.e. *diagnostic power.*

	Form A			Form B			Form C		
Problem No.	Right solutions	Difficulty value	C	Right solutions	Difficulty value	C	Right solutions	Difficulty value	C
	%	σ		%	σ		%	σ	
1	72	4·42	65 ± 14	79	4·19	73 ± 1	82	4·08	81 ± 5
2	69	4·50	69 ± 4	63	4·67	70 ± 4	63	4·67	71 ± 5
3	59	4·77	67 ± 5	62	4·69	56 ± 15	58	4·80	58 ± 20
4	55	4·87	81 ± 0	56	4·85	62 ± 1	54	4·90	49 ± 7
5	53	4·92	66 ± 4	49	5·02	58 ± 9	53	4·92	64 ± 8
6	48	5·05	54 ± 4	46	5·10	49 ± 0	45	5·13	55 ± 7
7	43	5·18	67 ± 3	40	5·25	58 ± 11	40	5·25	78 ± 18
8	34	5·41	80 ± 8	34	5·41	72 ± 15	37	5·33	78 ± 4
9	31	5·50	61 ± 6	31	5·50	46 ± 4	28	5·58	72 ± 2
10	22	5·77	75 ± 3	30	5·52	70 ± 2	27	5·61	83 ± 8
11	19	5·88	47 ± 9	21	5·81	82 ± 12	20	5·84	68 ± 13
12	19	5·88	88 ± 2	22	5·77	64 ± 5	20	5·84	64 ± 1
13	13	6·13	51 ± 8	11	6·23	94 ± 3	19	5·88	51 ± 1
14	11	6·23	70 ± 6	7	6·48	*	5	6·64	*
15	1	7·33	*	†	—	—	†	—	—

* Too few right solutions obtained to estimate value of C.

† Problem added similar to A 1.

Diagnostic value of each problem. There was, however, no surface indication that each problem was doing its fair share in the diagnosis of ability. The next question to be decided, therefore, was this:

If a certain problem is solved by x per cent. of a group, to what extent are the correct solutions produced by the *best* x per cent., as adjudged by their scores on the whole test?

If a problem were in perfect consistency with the purpose of the test, theoretically the most able x per cent. would solve it, while the other $(100 - x)$ per cent. would fail; on the other hand, if it were of no diagnostic value at all, the ratings of those who solved it would represent merely a random sampling of the whole group.

Thus a Coefficient of Internal Consistency, C, was derived, and its value found for each problem. The formula used was

$$C = \frac{2A - n - 1}{x - n} \times 100\text{[1]},$$

where n is the number of individuals tested, x the number correctly solving the problem, and A the average rank of these x individuals. Then, if the diagnostic value of a problem is zero, the x individuals are merely a random sampling of the whole group n, $A = \frac{n+1}{2}$, and $C = 0$, while for perfect consistency, $A = \frac{x+1}{2}$, and $C = 100$. Between these limits will lie the values of A and C for all problems whose solutions depend to a certain extent on reasoning ability, and the more nearly their coefficients approach to 100, the more valuable are they as test problems.

This criterion was applied to each problem in turn; by using data from two parallel and equal groups of candidates, two values of C were obtained for each problem, and thereby an indication of the reliability of the coefficient. Table I shows the average values of C, together with the deviation of each estimate from their average. For seventeen problems the coefficient was over 70, while for three it fell below 45; the agreement between each pair of estimates was fairly close in almost every case. As with other coefficients of association, these become less and less reliable as the difficulty of the problem diverged more and more from the average problem solved by 50 per cent. of the candidates, and it is doubtful how much value can be attached to the coefficients of problems solved by more than 80 per cent. or less than 20 per cent. On the strength of these results three problems were discarded for lack of diagnostic power, two more were added of exactly the same nature as the hardest problem (A 15), and, with the elimination of one other, the remaining forty-five were divided into three parallel forms A, B and C, as shown in Table I, the basis of arrangement being the σ value of difficulty, so that, finally, the individual problems of each form constituted a series of fairly equal steps from the easiest to the hardest problem, and the total difficulty of each form was the same[1].

---

[1] C really denotes the excess of the average rank A of the x individuals over the rank $\frac{n+1}{2}$ of a random sampling of the whole group n, as a percentage of the range between pure chance and perfect consistency, *i.e.* $C = \dfrac{A - \frac{n+1}{2}}{\frac{x+1}{2} - \frac{n+1}{2}} \times 100 = \dfrac{2A - n - 1}{x - n} \times 100.$

4. *Administration of the test.*

Below is reproduced Form B, together with the Practice Sheet, to give a clearer conception of the actual test. Each child is given a copy, and all answers are registered on the sheet together with the child's name, school, class, age and sex. Instructions are given as follows: "This is to see how well you can think out a problem. You have 15 problems to do; start at number 1 and work right through them all. It is no use trying to guess the answers; the problems are made so that anyone who guesses is almost sure to be wrong. They are not hard, but if you find one that you cannot do, even after several tries, put a X against it in the margin. Do not hurry; everyone will be allowed as much time as he wants. When you have finished, bring your paper to me and then go on quietly with other work[2]."

The three problems on the practice sheet are then taken by examiner and children together, making quite sure that the problems and the methods of recording the answers are fully understood by all. The instructions are read once again, and the children are given opportunity to ask any final questions, after which the actual test is begun.

It is found that the atmosphere created by this introductory period is of great importance; there should be an absence of any hurry or severity, a general feeling of cheerfulness, and complete understanding of what is to be done, as well as interest in the proceedings and a sense of escape from the ordinary class-room routine. In the few cases where, owing to lack of time or other uncontrollable causes, this atmosphere could not be created, or was destroyed before the test was finished, the results were obviously unsatisfactory.

---

[1] Later results showed that Form B was slightly the hardest, and that in different schools and different districts the relative difficulty of the problems altered slightly, generally for no apparent reason. But as long as unlimited time was allowed, so that each child could attack every problem, their arrangement in exact order of difficulty was of secondary importance.

[2] Individual times taken to complete the test varied from 17 minutes to 70 minutes, but no useful work was done after one hour. The full period allowed for the administration of the test, including the practice period, should be one hour and a quarter.

PRACTICE SHEET.

1. I am older than Mary and younger than John.

Draw circles round the numbers of the two true sentences among the following:

1. John is younger than Mary.
2. Mary is younger than John.
3. Mary is older than John.
4. I am the oldest of the three.
5. Mary is the youngest of the three.

2. In my garden grow several different kinds of flowers; some smell very sweet and some have no smell. Those which smell sweet are roses, lilies, wall-flowers, carnations and hyacinths, and the other kinds are daisies, snowdrops and dahlias. Snowdrops flower in the winter, hyacinths and wall-flowers in the spring, dahlias in the autumn and all the others in the summer. I want to pick from my garden in summer a bunch of three different kinds of sweet-smelling flowers.

Underline the three flowers among those below that I shall pick:

Carnation, dahlia, daisy, hyacinth, lily, rose, snowdrop, wall-flower.

3. Mrs Brown has two boys, Tom and James. They are sometimes very naughty, but Tom always tells the truth. A cake disappeared from the cupboard one day; both boys said that they knew nothing about it, and James suggested that the dog had stolen it. Next day Mrs Brown found the cake again in the cupboard, half eaten. What had happened to the cake?..

GROUP REASONING TEST.

FORM B.

Name.. *Class*...................

School.. *Age*.......................

.... *Boy or Girl*...........

1. A burglar entered my room last night. The windows were all securely fastened on the inside, and the fastenings and the window-panes had not been touched. There is no chimney, and the only door in the room leads to the corridor, and was locked, the key being left on the outside. There is no other opening in the walls or the ceiling or the floor by which the burglar could have entered.

How did he get in? ....

2. A laundress had customers whose names were—BLAKESLEY, CLARK, CLAUDE, ELAM, ELLIS, LARGE, LLEWELLYN, SLOANE, SMITH, and STRONG. She had a sheet marked with a name almost rubbed out; the first letter and the last two could not be

read, and the second letter was L, and the third was A, and there were five letters altogether.

To whom should she return the sheet? ....

3. There are two kinds of presents that I have bought for Christmas. Those of the first kind are cheap; the others, which are dear, are a book, a watch, a muffler, a walking stick, and a tie. Those of the first kind are cigarettes, chocolate, a doll, and a pipe. My father and mother are to have the muffler and the book. My brother, who is 20 years old, does not smoke and already has a watch and a walking stick. I want to give him one of the cheap presents and one of the other kind.

Draw a line under each of the two things below that I shall give him:

Book, watch, muffler, stick, tie, cigarettes, chocolate, doll, pipe.

4. Kingston is west of Newtown, and Newtown is west of Thorpe.

Draw circles round the numbers of the two true sentences among the following:

1. Thorpe is north of Newtown.
2. Newtown is south of Kingston and Thorpe.
3. Kingston is west of Thorpe.
4. Kingston is east of Thorpe.
5. Thorpe and Newtown are east of Kingston.

5. Peter has a half holiday on Wednesday and Saturday afternoons and a whole holiday on Sunday. I am at work all day except on Monday, Wednesday, Friday, and Sunday, and the shops are shut on Saturday afternoons and all day on Sunday. I want to take Peter to the tailor to get him a new suit.

Write down the day on which it will be best for us both to go, and write "morning" or "afternoon" after it, to show at what time we had better go.

....

6. Tom's teacher has told him that he must bring a pen or pencil to school, but that he must not be late. He has twopence, but no pen or pencil; a pen costs threepence, and a pencil costs a penny. It takes Tom 5 minutes to get to school, and 5 minutes to get to the shop and back, and there are only 15 minutes before school time.

Draw circles round the numbers of the two sentences which tell what you think Tom must do.

1. Tom must go to school at once and buy a pen this evening.
2. Tom must buy a pencil and not a pen.
3. Tom must go straight to school without going to the shop.
4. Tom must go to the shop and then go straight to school.
5. Tom must buy a pen and then run all the way to school.

7. Edith is fairer than Olive but Edith is darker than Lily.

Draw circles round the numbers of the two true sentences among the following:

1. Olive is fairer than Lily.
2. Olive is the fairest of the three.
3. Lily is the fairest of the three.
4. Edith is not so dark as Olive.
5. Olive's hair and Lily's hair are exactly the same colour.

8. Ten children, whose names were A, B, C, D, E, F, G, H, I, and K, competed

for a prize in English Essay writing. Writing, spelling, punctuation, size of vocabulary, grammar, and composition were to have equal weight in the determination of the best essay. The judges reported as follows:

B, E, G, H, and K write badly.
C, E, F, H, and K have made no mistakes in spelling.
B, F, and G have a large vocabulary.
C, D, I, and K use bad punctuation.
A, B, E, G, H, and I have made mistakes in grammar.
A, F, and I show the best composition.

To which child should the prize be given? ....

9. I am making a collection of flies, moths, bees, spiders and grasshoppers. Now flies, grasshoppers and bees all buzz, while spiders and moths make no noise. Moths, flies and bees can all fly, while grasshoppers and spiders cannot fly. I have found an insect that cannot fly, but sits on the ground and buzzes.

Draw circles round the numbers of the two true sentences among the following:

1. My insect is a spider.
2. My insect is a grasshopper.
3. My insect is a moth.
4. My insect might be a spider if it did not buzz.
5. My insect is not a grasshopper.

10. A pound of meat should roast for half-an-hour.
Two pounds ,, ,, ,, ,, three-quarters of an hour.
Three ,, ,, ,, ,, ,, one hour.
Eight ,, ,, ,, ,, ,, two hours and a quarter.
Nine ,, ,, ,, ,, ,, two hours and a half.

From this you can find a simple rule by which you can tell by the weight of a piece of meat how long it should be roasted. Find this rule, and write down how long you should roast a piece of meat weighing 12 pounds. ....

11. A man is painting a sign for a circus. He can write either: (1) "WILD ANIMALS," or (2) "WILD BEASTS," or (3) "MAN-EATING TIGER," or (4) "LIONS, TIGERS, & BEARS," or (5) "LIONS & TIGERS," but he must take up as little space as possible. If every letter and every space between two words takes up the same amount of space on the sign, write down here the number of the phrase that he should use,.................... and cross through the phrase among those above which would be the worst for him to use.

12. A man who lived a little way south of London bought a ticket at his station, and received some change after giving the clerk a £1 note and a 10/- note. He travelled to London and was seen there going into Euston Station, from where trains run north to Scotland and north-west to Holyhead. The trains to Scotland stop on the way at York, and those to Holyhead stop on the way at Crewe. The fare from his station to London, York or Crewe is less than 25/-; to Scotland or Holyhead it is more than £2. The man did not get into the train going to Scotland.

Draw circles round the numbers of the two true sentences among the following:

1. He was going to York.
2. He was going to Crewe.
3. He did not intend to go to Crewe.
4. He passed through York in the train.
5. He would not reach Holyhead.

13. The hands of my clock, which is an old one, are of equal length. Yesterday I wished to do business with a man who is only in his office from 12.30 till 1.30 p.m. each day, and may be five minutes early or late in either coming or going. I looked at the clock and thought that the hands stood at five minutes past 12. It takes 27 minutes to walk to the office, and I started at once. On the way it occurred to me that I might have read the clock wrongly, and that the hour hand was really where I had thought the minute hand stood. When I arrived at the office it was shut. How long did I have to wait to make sure that I had not arrived before the office opened?........................ If it did not open at all, what time was it when I started from home? ....

14. A man who was travelling with a dog, a cat, a rabbit and a parrot that could not fly, came to a river. There was no bridge, and the current was too strong for any of the animals to swim across, but the man found that he could wade across carrying two animals at a time. He could not leave any animal alone on the far side of the river except the dog, for there was a fox prowling round over there which would have killed any of the others unless the dog was there to frighten him away. Moreover, the dog would kill the cat or the rabbit if left alone with them, and the cat would kill the rabbit or the parrot.

However, by making three journeys across, each time carrying over two animals and bringing back one, the man was able to continue his journey with all his animals unharmed.

Write down the name of the animal required in each space below to give all details of his journey.

1. Carried the..............and the..............across, brought back the...........
2. Carried the..............and the..............across, brought back the...........
3. Carried the..............and the..............across.

15. A builder wanted exactly 8 quarts of water for mixing his mortar, so he gave his boy a 5 quart pail and a 7 quart pail, and told him to get 8 quarts from the well. The boy, who was very clever, managed to measure exactly 8 quarts without guessing the amount, and using nothing except these two pails. Below are the different things that he did with the pails, but not in the order in which he did them. Put a "1" at the left-hand side of the first thing he did, a "2" at the left-hand side of the second thing he did, a "3" at the left-hand side of third thing he did, and so on, putting a "7" at the last thing he did:

Number ....filled up the 7 quart pail from the 5 quart pail.
............filled the 5 quart pail.
............emptied the 7 quart pail.
............filled the 5 quart pail.
............emptied the 5 quart pail into the 7 quart pail.
............filled the 5 quart pail.
............emptied the 5 quart pail into the 7 quart pail.

5. *Results of testing.*

(*a*) *Children tested.* All three forms of the test were given to nearly 300 children of grades VI, VII and VIII, drawn from two New Haven (U.S.A.) Elementary Schools; the range in age was from eight pupils under 11 years to sixteen pupils over 15 years. Table II gives the averages and variability of the scores in each grade, and also the unreliability of these averages. As far as possible the practice effect, which quite distinctly existed, was eliminated from the total average scores by giving the tests A, B and C in a different order to the different sections of each grade; thus grade VII_A did the tests in the order A, B, C, grade VII_B in the order C, A, B, and grade VII_C in the order B, C, A. The final average score found for any one test will therefore be slightly higher than that found if this test had been done by all pupils without previous experience of either of the other two.

TABLE II. *Summary of results for* 289 *children in Elementary Schools in Connecticut, U.S.A.*[1]

	Form A			Form B			Form C			Total		
Grade	VI	VII	VIII	VI	VII	VIII	VI	VII	VIII	VI	VII	VIII
Av. score	7·54	7·72	9·83	6·26	7·62	9·32	6·70	6·70	9·03	21·0	22·5	28·5
σ (distribution)	2·68	2·37	2·04	2·45	2·49	1·99	2·43	2·61	2·70	6·75	6·18	5·64
σ (average)	0·23	0·23	0·23	0·26	0·23	0·23	0·26	0·24	0·31	0·71	0·56	0·64
No. of pupils	90	122	77									
Av. age (years)	12·2	12·9	13·7									

TABLE III. *Summary of results for pupils in various types of English schools.*

	Elementary Schools			Central School		Girls' Private Schools	
						X	*Y*
Class	V	VI	VII	I and II	V	IV	V
Av. score	5·76	6·02	7·96	7·48	11·54	10·15	10·76
σ (distribution)	2·35	2·49	2·53	2·47	1·66	3·35	2·73
σ (average)	0·20	0·24	0·25	0·29	0·45	0·75	0·54
No. of pupils	141	107	105	73	13	20	25
Av. age (years)	11·6	12·2	13·2	12·2	15·5	13·3	15·8

A year later the test was given, with the small alterations in wording and phrasing necessitated by crossing the Atlantic, to various schools in Bristol. Of three Elementary Schools tested, *A* was a mixed school drawing on a poor district with a shifting population, *B* was the girls' section of a school in a more prosperous suburb, and *C* was a small three-roomed girls' school where the standards tested, V, VI and VII, all worked together in one room. The test was also given to seventy children in the two lowest classes of a Central School, to a small group of boys at the top of the same school, and to small groups from two private girls' Secondary Schools of high standing. Table III summarizes these results.

---

[1] The fixing of normal scores by grades or standards rather than by age is admittedly not ideal, but there seemed in this case no other alternative. It is hoped, however, that it may be possible to publish later age norms for London schools.

(*b*) *Consideration of the scores.* Tables II and III cannot, of course, be considered as giving established norms, but they are quoted as an indication of the range of scores and averages for different classes and ages. They should not be used in strict comparison of the ability of American and English children, even with restriction to two fairly typical cities of New and Old England, for they do not represent the same type of sampling in both cases. In the first place, the average social status of our Elementary School children is lower than in America where the claims of democracy bring together the very poorest with many who in England would attend the junior forms of public or private Secondary Schools. Secondly, here, as in America, the Secondary Schools drain away some of the best brains of the upper standards, but in rather different ways; the American Elementary Schools retain almost all children until 12 or 13 years, even though a few enter the 'High School' earlier than the normal age of 14, while in England scholarships and free places may remove the children somewhat earlier from the Elementary School, and this effect is increased by the existence of Central Schools. Thirdly, in the Bristol Elementary Schools there was scarcely a single child to be found over 14 years by more than a month or two; the average age in the top standard is about six months younger than that in the highest grade of an American Elementary School, which makes comparison class by class difficult; and year by year impossible, for the older children.

The only comparison that can fairly be made is that the top American grade is distinctly in advance of the top English standard, but that, age for age, or in relation to length of school life, neither can claim much advantage. The standard deviations also are very similar[1].

The obvious superiority of the Central School children over a class of similar age in the Elementary School shows that the former are a selected group in more than name; the most noticeable feature of the Private School results, beyond their superiority to all but the little group of picked boys in the Central School, is their distinctly large standard deviation from the average score.

---

[1] It is interesting to note a point that must seemingly be put to the credit of the New Haven school system. The reading of a typical class list in a New Haven school is no easy matter for an untravelled Briton, for more than 50 per cent. of the population consists of foreign labourers and factory hands, Italians, Greeks, Russians, Poles, etc., whose children may have been born in America, but whose language in the home is still European, and who would not rank very highly in the social scale of their native countries.

(*c*) *Reliability of the test.* The reliability of the test is fairly satisfactory, although admittedly below that of the original individual test[1]. Correlation coefficients between the three Forms A, B, and C varied between 0·50 and 0·66 for separate class groups, with an average of 0·61. It must be remembered that the three forms of the test were given in a different order to different sections of each grade, so that one-third of the children would have had two periods of practice before doing Form C, one-third one period, and the rest none. This would lower the correlation between forms to a considerable extent, and, moreover, each group was highly selected; for one undivided class of forty-eight pupils the correlations were as follows: A : B = 0·65, B : C = 0·68, C : A = 0·77. The correlations of any one form with the total on the three forms varied between 0·81 and 0·92 for separate grade groups of 50 to 100 children.

(*d*) *Comparison with other measures of ability.* Apparently, then, the test gives a fairly stable measure of something, but exactly what this quantity is raises a bigger question. That reasoning ability is involved to some extent seems obvious, and that reasoning ability is akin to general intelligence will hardly be disputed. Yet the data show that the test does not measure exactly the same thing as is measured by intelligence tests, nor, apparently, does it measure precisely what teachers understand by intelligence. It has not been possible to compare it with the Binet tests or any other individual test, but the following correlations with other group tests have been found:

With Pressey Group[2] Test for 107 children from 1 grade, $r = 0{\cdot}44$. P.E. $= \pm\ 0{\cdot}058$.

With West Riding Group[3] Test for 24 children from standards V, VI and VII, $r = 0{\cdot}57$. P.E. $= \pm\ 0{\cdot}09$.

With West Riding Group Test for 23 children from form IV, Secondary School *X*, $r = 0{\cdot}78$. P.E. $= \pm\ 0{\cdot}055$.

---

[1] For children of all ages scores on two parallel halves of Burt's individual test gave a correlation of 0·93; the short list correlated with the full list to the extent of 0·89 for a mixed group of 108 children, and to the extent of 0·75 to 0·78 for each standard separately. (See Burt, *op. cit.*)

[2] See S. L. and L. C. Pressey, *Introduction to the Use of Standard Tests*, Harrap, p. 160.

[3] T. P. Tomlinson, published by Hodder and Stoughton, London.

With West Riding Group Test for 18 children from form IV, Secondary School Y, $r = 0{\cdot}85$. P.E. $= \pm\ 0{\cdot}044$[1].

Teachers' estimates show still less agreement; this is hardly surprising in the case of the English schools, for the teachers had for the most part had no previous experience of tests or testers and their requirements and terminology, and the idea of a general, separable factor of intelligence was somewhat new to them. For standard groups of thirty to forty children the correlation between test results and teachers' estimates varied between 0·25 and 0·68, averaging 0·48; obviously the reliability of such estimates is too doubtful to enable them to be used as final indications of the reliability of the test; they are, however, quite consistent with the view that the tests are good measures of intelligence.

Correlations with performance in school subjects were less variable. A class group in an American school showed correlations with the different sections of the Lippincott-Chapman Classroom Products Test[2] as follows:

Reasoning Test score:	arithmetical problems,	$r = 0{\cdot}56$.	P.E.	$\pm\ 0{\cdot}05$
,, ,, ,,	arithmetical fundamentals,	$r = 0{\cdot}51$.	,,	$\pm\ 0{\cdot}06$
,, ,, ,,	reading tests,	$r = 0{\cdot}53$.	,,	$\pm\ 0{\cdot}06$
	Total score,	$r = 0{\cdot}62$.	,,	$\pm\ 0{\cdot}05$

In the English Central School class groups gave correlations of

0·45–0·50 with mathematics,
0·41–0·45 with science,
0·34–0·44 with English,
0·48–0·58 with total class position[3].

A closer correspondence with mathematics and science than with general English work is to be expected, and the same result was found on examining the scores in individual parts of the West Riding Intelligence Test; correlation with 'Incongruities' was as high as 0·70, with 'Arithmetical Problems' 0·51, and decidedly lower with the more linguistic sections, such as 'Analogies.'

(e) *Effect of age.* No reliable norms can be given at present for age-groups, since the 12-year old group is the only one that could be considered at all representative, but there are various indications of the effect of age on the power to reason.

---

[1] The reason for the higher correlation in Secondary Schools is not clear; it may simply be due to greater ability, or to the type of teaching, either of which might tend to lessen the difference in the method of attack on tests with limited and unlimited time (see below under 'Time factor')—or, again, it may be due to greater heterogeneity of ability within one class.

[2] Published by Lippincott Co., Philadelphia.

[3] With groups of this size and coefficients of this magnitude the P.E.'s range from ±0·07 to ±0·10.

In American schools a noticeable feature in almost every class was the tendency for the younger boys to register the higher score (see Table IV). The tendency was present in many English classes, but to a less marked extent, indicating a better system of school grading, though one by no means perfect. The selection of the cleverer children for Central and Secondary Schools may also have had its effect in removing the brightest of the younger children rather than the older, duller individuals. It will be seen from Table IV also that a 14-year old boy of Grade VII shows greater ability than a 14-year old boy from Grade VI; reasoning ability is clearly not the product of age alone.

TABLE IV. *Showing typical examples of average scores for year-groups in two adjacent classes of boys.*

	Grade VII		Grade VI	
Age	No.	Av. score	No.	Av. score
15	2	6·50	—	—
14	12	6·67	4	5·50
13	15	6·67	12	5·25
12	24	7·88	12	7·17
11	6	9·17	12	7·25
10	2	9·00	2	6·50

The most reliable estimate of the increase of this ability with age will be found by comparing the scores of children of one year-group in one class with those of the next higher year-group of the class above. The mean value of this measure of annual mental growth, derived from eight estimates in American schools, is 1·50 marks out of the possible maximum of 15; in English schools it is 1·25. This relates only to annual growth between 12 and 14 years in America, and 11 and 14 years in England, and during this rather limited period there is no evidence of steady acceleration or retardation of rate of growth.

For the whole 12-year group the standard deviation is 2·6, and for the whole 13-year group it is 2·8; these two are the most representative age-groups tested; in each case the standard deviation is nearly twice the average annual increment. Burt has found that in the case of school subjects the standard deviation and the annual increment are about equal; it is obvious, therefore, that reasoning ability, as measured by this test, is more variable among children of any age-group than the results of school training, and also, bearing in mind the average annual increment in score, that chronological age counts for less in the test-result than sheer native ability.

(*f*) *Age limits.* Standard IV, with an average age of 10½ years, produces one or two zero scores; in the American grade IV, with an average age of 10, too many children had difficulty with reading and recording to give scores of any value. The lower limit of utility for the test is fixed therefore at standard IV or 10 years for England, and at grade V in America.

The upper limit is less definite. There was considerable deviation between the boys of 15 and 16 at the top of the Central School, and no one attained full marks; among the girls in private Secondary Schools the test showed distinct diagnostic power for 16-year olds, although the average age of those tested was 18 months below this. It seems safe to say that the test is of good diagnostic value up to 15 years, and of some value in the following year; probably this limit could be extended by imposing a time limit, but it seems preferable to accept a lower age limit than to change the character of the test by introducing an entirely new factor.

(*g*) *Effect of sex.* There appears to be no very marked sex difference operative; on the whole, any difference is to the credit of the boys. It seems not improbable that the system of teaching may have a decided influence upon the results; in the mixed classes of American schools, taught almost entirely by women, there was a marked superiority of boys in the highest grade, but the testing was too restricted to show if this was merely a chance finding; in the other grades there was no significant difference. In the Bristol schools tested, where the boys are taught by men and the girls by women, there was a more marked sex difference, the boys being ahead on the average by 0·7 of a point out of a possible maximum of 15; in the mixed classes of the Central School at a similar age the girls surpass the boys by 1·5 points; there was no evidence of a selective factor operative here, and girls and boys were about equal in numbers.

Although there is not sufficient evidence to warrant any definite conclusion, it is of interest to record the indication of a small sex difference in favour of the boys accentuated by the teaching in separate rooms of boys by a man and girls by a woman.

(*h*) *Comparison of the group with the individual test.* The lower age limit indicating the bound of utility of the group test shows that it is distinctly more difficult than Burt's oral test, which he uses successfully with 7-year olds. A comparison of the ease of solution of the same problem in the two forms corroborates this in the few cases to which it can be put to the test. For example, Burt's problems Nos. 7 and 18 (group test A 1 and A 6) were solved by 90 per cent. and 75 per cent. respectively of 10-year olds tested individually, and by almost exactly the same percentages tested in groups. But the latter 10-year old group was distinctly in advance mentally of Burt's unselected group, since only those in the higher grades were tested. Moreover, Burt's problem No. 31 (C 10 group test) was solved by 58 per cent. of his individually tested 11-year olds, but only by 45 per cent. in the groups tested, although here again the dullest 11-year olds may have been missed in the group testing. In cases where the problem was slightly altered in wording there was a similar increase in difficulty, and the addition of sentences from which the solution has to be selected had the same result.

This difference may be due to several factors; the actual recording of the solution in written rather than oral form may be a contributory cause; the apparently greater complexity due to the shaping of the group test problems to avoid the possibility of guessing is another and a more serious factor. Moreover, no help is given in the group test with the reading or understanding of the problems, and there is no possibility of a second or third trial after initial failure, as in the individual test.

In Burt's opinion the most valuable part of his test lay in the questions following the solution which showed the steps taken by the child to reach his solution. When the examiner's contact with the examinee is reduced to a study of his solutions recorded in black and white, this more intimate exploration of the child's mentality is impossible. The re-shaping of the questions obviates the worst dangers of this limitation, but it is only to be expected that the immense saving of time in group testing is effected at the cost of greater certainty of diagnosis.

Finally, the range in score on one form of the group test is from 0 to 15, with no half credits possible; in the individual test the score may be accumulated in $\frac{1}{4}$ points to the total of 17 points on the short list alone. The individual test, therefore, is an instrument calibrated with over four times as fine a scale as the group test, and this fact alone may account largely for the lower reliability of the group test noted before.

(*i*) *Time factor.* Most group tests of intelligence, while requiring accuracy, require also considerable speed of working, and unless ability is proportional to speed in mental processes, the quick worker may score highly even though his percentage accuracy is below average, while the score of a slow worker may represent his rate of working rather than the quality of his work[1]. This point is raised very commonly as an argument against the validity of group tests, and there is not yet sufficient evidence that many intelligence tests do not make too great a demand on speed of mental reaction and too little on the power of constructive thought. To construct a test without a time limit which shall tax to the utmost the powers of the testees grows increasingly difficult as their age increases; for adults it becomes almost impossible to find material of sufficient complexity which is yet of equal familiarity to each person tested, and it is difficult even with the older children.

In practice, however, occasions demanding full exercise of reasoning powers do not in general demand also great rapidity of mental functioning; in the reasoning process speed seems of secondary importance. The principle adopted in this test is, therefore, that each individual shall work at his own pace, with time at his disposal to solve all the set problems that are within his capacity to solve; the limit of his score is to be fixed by quality and accuracy and not by speed of working.

Various points of importance appear in this connection; they will be taken up one by one.

1. *Relation between score and speed of working.* With various groups the time taken by each individual over part or the whole of the test was registered. Since in some cases much time might be taken over one or two of the most difficult problems with no result to show for it, the time spent on the first five problems was used to give a measure of the speed of working. Correlating this speed with the score, there appears to be no steady relationship between the two, the coefficient varying in different groups from − 0·29 to + 0·35, in the majority of cases being negative. With teachers' estimates of ability the speed correlated very low also. Correlation of time taken over the whole test with score in the case of one or two groups of older children gave similar results.

A study of the composition of various speed groups was made, as shown in Tables V A and B. Divisions were made according to speed into groups 'very quick,' 'quick,' 'average,' 'slow,' and 'very slow'; each group was then cross-classified by score as 'very good,' 'good,' 'average,' 'poor,' and 'very poor.' Again there is no evidence of any relationship between score and speed: each speed group is distributed approximately normally as to score, and *vice versa*.

2. *Relation between speed and accuracy.* Often an answer seemed to

---

[1] Recent work, such as Bernstein's "Quickness and Intelligence," Monog. Suppl. to this *Journal*, vol. III, No. 7, seems to suggest that ability and speed are factors very near akin.

show a right solution wrongly indicated, or a wrong reading of the problem. To investigate this effect some groups were re-tested several days after the first trial, with a special warning against careless work[1]. The results showed a rise in score and in speed, but no great difference in relative ranking, correlations between the first and second scores being in all cases about + ·80.

TABLE V. *Showing distributions in speed relative to score for groups from* (A) *Standard VII,* (B) *Central School Classes I and II.*

TABLE V A.

	Very good	Good	Average	Poor	Very poor	Total
Very quick	0	1	3	1	0	5
Quick	2	5	6	8	3	24
Average	5	7	20	6	1	39
Slow	1	6	7	5	1	20
Very slow	0	1	3	1	0	5
Total	8	20	39	21	5	93

TABLE V B.

	Very good	Good	Average	Poor	Very poor	Total
Very quick	0	0	4	2	0	6
Quick	1	1	5	3	1	11
Average	2	5	25	1	4	37
Slow	1	4	4	1	0	10
Very slow	1	0	3	2	0	6
Total	5	10	41	9	5	70

A few interesting cases appeared of problems solved rightly at the first trial and wrongly at the second, 3 children out of a group of 73 making no fewer than three such changes each, and 4 more children making two each. All of these had low average scores, were moderately quick in their first performance, and decidedly quick in their second, with the exception of one individual who was in both trials among the last six to finish, bringing up his second score to the same figure as before by solving correctly three new problems in place of his three new failures. Guessing does not account wholly for these lapses, and lack of care seems an inadequate reason. The result may be due to the same factor that was suggested before as a cause of the lower reliability of the group test, namely, that in the exercise of reasoning there is no one process alone involved, but many, and that the measurement of reasoning power simply by the written end-products of the solution of fifteen problems is likely to show anomalies and irregularities due partly to the effort to measure a complex function by a simple instrument of only fifteen units of scale.

---

[1] Enquiries showed that little or no effort had been made by the children to compare solutions after the first trial, and so to correct individual mistakes.

3. *Speed and improvability.* Out of the group re-tested were picked the quickest twelve and the slowest twelve; their improvement in the second test is shown in Table VI.

TABLE VI. *Showing improvability in the* 12 *slowest and the* 12 *quickest workers out of a group of* 73.

Slowest Group			Quickest Group		
Score			Score		
1st trial	2nd trial	Improvement	1st trial	2nd trial	Improvement
12	13	1	9	13	4
12	12	0	9	10	1
10	12	2	9	8	−1
10	11	1	8	11	3
10	11	1	8	10	2
9	13	4	8	8	0
9	12	3	6	5	−1
9	12	3	5	7	2
8	10	2	5	4	−1
7	9	2	4	11	7
4	9	5	4	9	5
4	6	2	4	8	4

In the quick group there is evidence of quick work in the first trial at the expense of accuracy, greater care in the second trial raising the score very considerably. There also seems evidence in this group of lack of care persisting in the second trial in some cases, there being no fewer than three lowered scores.

'Slow and sure' seems to be the character of the 'slowest' group throughout.

4. *Diagnosis by comparison of results in tests of limited and unlimited time.* Other standard intelligence tests[1] were given to some groups, and a special study was made of individual cases which, by considerable difference of rank in the Intelligence Test and the Reasoning Test, were chiefly responsible for the low correlation between the two. These cases fell into two categories:

Class A, typified by a high I.Q. in the Intelligence Test, and an average or low score in the Reasoning Test.

---

[1] The West Riding Group Test was used in most cases; with a few the Simplex Group Test was also tried, but this seemed less satisfactory. The I.Q. values were taken from the norms quoted by the authors; judged by these the Elementary School group tested was distinctly inferior, showing an average I.Q. of only 89·2.

Class B, typified by a low I.Q., but showing considerably more ability in reasoning.

The results will be most clearly shown by reference to individual records in the West Riding Intelligence Test, with the I.Q. so determined, and in the Group Reasoning Test.

M. M. Std. V, 11 yrs; I.Q. 127, 2nd in group of 30; accuracy 64 per cent. Reasoning ability average, 10th in group.

This girl attacked many more items in the Intelligence Test than most of the group, and even with her low standard of accuracy (64 right out of every 100 attempted), she scored enough to rank second. In the Group Reasoning her score was barely average, and yet she finished fourth in point of time. Purely on test results she appears to have ability, but is too quick over her work, and where speed cannot compensate for accuracy, she is handicapped.

The teacher's estimate is in close accord with this diagnosis: she is registered as "very good, but careless."

E. N. Std. VII, 13 yrs; I.Q. 86, 12th in group; accuracy 64 per cent. Reasoning score 10, 1st in group.

Here is an individual who shows in the unhurried test decided ability and more than normal accuracy, and yet her performance in the timed test is poor and inaccurate. We can only conclude that, if allowed to work at her own pace, she is sound and careful, but that to her mentality hurry is fatal. Again there is corroboration from her teacher: "good, but slow and stolid."

S. C. Std. VI, 13 yrs; I.Q. 75, 22nd in group; accuracy 31 per cent. Reasoning score 7, average, 5th in group.

Another girl of the same type as E. N. but showing still more strongly the fatal results of hurry; in the Reasoning Test she was nearly the last to finish, but working at her own pace she did fairly well. Her teacher says: "Good, but unsteady." We would suggest that the 'unsteadiness,' if analysed, would be found to be due to attempts to work too quickly.

Cases could be multiplied, but their study must be summarized shortly.

Much can be derived from the results of a Group Intelligence test beyond the mere final score. The percentage accuracy and a study of where marks are lost and gained may reveal temperamental factors. Lack of accuracy is shown especially in an irregular scoring of marks among easy and hard questions alike. An additional comparison with a test unlimited as to time, given under conditions which obviate hurry, shows to what extent each individual is affected by working against time. It may be possible to conclude of some individuals that they must, for the present at any rate, be left to work at their own slow rate if any satisfactory work is to be done; of others that they must be made to work more slowly and carefully; of still another group, whose answers are slow but very accurate, that greater speed might be encouraged with a fair chance of maintaining a good standard of accuracy. Thus, a combination of the two types of test may indicate more about the probable quality and character of work of the child entering school than either test by itself could show.

6. *Conclusions.*

The general results obtained from the tests show that a test without a time limit is not only practicable in administration but also of distinctive use in diagnosis.

The tests can be used with children between 10 and 16 years; the time taken by each child is from 20 to 60 minutes, except in very extreme cases; the administration is simple, the test being preceded by a regulated practice period, and the scoring is purely objective.

All three forms of the test are approximately of equal difficulty. The correlation between the forms is fairly high in comparison with similar correlations for other tests, but the reliability is distinctly lower than in Burt's original individual test. This seems due partly to the relatively coarse calibration of the group scale, and partly to the effort to estimate the complex process of reasoning by a simple measure of the end results alone, but mainly to the use of the group procedure in which of necessity no individual supervision or assistance can be given to the child in mastering the mere technique of the test.

The test shows no appreciable difference between English and American children of the same age, either in average ability or in deviation from the average; this applies merely to very restricted districts in each country.

Yearly growth in reasoning ability between 11 and 14 years gives steady increments in score, the average yearly increment being only one-half of the standard deviation for any year-group; this corroborates Burt's conclusion that age is a less potent factor than innate ability in determining reasoning ability.

There is no steady sex difference revealed, but merely an indication of the better development of reasoning power in boys taught by men than in girls taught by women.

While correlation with teachers' estimates proved very unreliable, correlation with school attainments was fairly constant, being highest (0·45 to 0·56) with arithmetic and mathematics, and next highest with reading; with general school work the correlation averaged 0·55.

It is possible that the test may measure something rather different from that measured by tests of general intelligence of the more usual type. It is suggested that the difference is due, partly at any rate, to the absence here of the necessity for hurry.

There is no relationship at all between score and rate of working. While some quick workers forfeit good scores through inaccuracy, and will score more highly if made to take more care, slow workers seem generally to work to the limit of their ability.

A second trial shows a distinct practice effect, which persists to a lesser extent through a third trial; the normal increase in score in the second trial is varied by occasional interesting cases of lower scoring or a substitution of wrong solutions for problems originally correct.

Combined with the ordinary type of time-limited test, the Group Reasoning test proves useful in the diagnosis of the character as well as the general quality of individual work, and can be used to indicate the advisability, or otherwise, of speeding up or slowing down the rate of working in individual cases.

(*Manuscript received* 26 *December*, 1925.)

THE PSYCHOLOGY OF CONATION AND VOLITION[1]

By F. AVELING.

(From the Psychological Department, University of London, King's College.)

I. *State of the Question* (pp. 339, 340).
II. *Spontaneous Distinction between Voluntary and Involuntary Actions and Trains of Thought* (pp. 340, 341).
III. *The Concept of Conation and its Applications* (p. 341).
IV. *The Origin of the Concept of Conation in Experience:*
  (*a*) *Theoretical* (pp. 341, 342).
  (*b*) *Evidential* (pp. 342, 343).
V. *General Principle of Conation* (pp. 343–345).
VI. *Principle of Instinctive Conation* (pp. 345, 346).
VII. *Special Characteristics of Choice* (pp. 346, 347).
VIII. *Principle of Volition:*
  (*a*) *In 'Desire'* (p. 348).
  (*b*) *In 'Choice'* (pp. 348, 349).
IX. *Relation of Value to Motive* (pp. 349, 350).
X. *Distinction between Conation and Volition* (pp. 350, 351).
XI. *The Intuited and the Conceived Self* (pp. 351, 352).
XII. *The Concept of Freedom* (pp. 352, 353).

I. *State of the Question.*

COMPARED with that of 'Intelligence,' and even of Emotion, the state of the psychology of Volition is a very unsatisfactory one. There is much speculative theory and little fact to support it. This is due, in the main, not only to the difficulties inherent in the investigation of Will-process, but also to the fact that Will—and, indeed, Conation in general—has been confused with the pathic, or affective elements entering into such complex states as those of desire and aversion. The orectic, conative or appetitive states have been made to include, or consist in, emotions. One famous account of 'Willing' reduces it to a cognitive anticipation, derived by way of image from past experience, of an action to be carried out; while yet another, no less famous, makes of it no more than a particular kind of fusion of elementary emotions.

---

[1] Communicated to Section J, British Association (Southampton, 1925).

Further, ethical considerations have been deeply involved in the treatment of problems concerning the Will; and for many centuries these problems were attacked and investigated from the moral standpoint rather than from the psychological one.

Moreover, until comparatively recent times, when dealt with psychologically at all, the question of Volition was handled—as were all other psychological questions—on the metaphysical plane; and it is largely due to the persistence of a metaphysical atmosphere in psychology that the whole subject has remained obscure.

The tendency to employ abstract terms rather than concrete ones, and to reify the abstractions which they denote, still further obscures the matter; and at the present day the terms are used by different authors in so many conflicting and confusing meanings that the psychology of Conation and Volition appears to be in an almost chaotic condition.

It is the aim of the present paper to attempt an unambitious statement of the elementary psychology of Conation and Volition, basing such statement upon facts which have been introspectively observed, in experimental conditions, with regard to so-called "Will-acts."

II. *Spontaneous Distinction between Voluntary and Involuntary Actions and Trains of Thought.*

It will be readily admitted that there is no difficulty in distinguishing between what we call a voluntary action, or train of thought, and an involuntary one. Not only do we not hold ourselves responsible for reflex and truly impulsive movements, for the images and ideas which arise unbidden in our minds (as in dreams or states of revery); but, quite apart from the sense of responsibility, we discriminate between our actions and thoughts as being willed or not willed. I will; and the action willed follows. I will; and my thought moves more or less logically towards the end desired. Behind this distinction we cannot go; it seems to be ultimate. If at all, it is *in* the characters of the two kinds of action or thought that the reason for the distinction must be looked for. Though all are indeed ours, in the sense of belonging to the same phenomenal consciousness, willed actions and thoughts appear to be ours to a peculiar degree and with a certain peculiar warmth of intimacy. They are actions and thoughts which follow on *effectual desire.* If we leave out of view the notion of effort connoted by the term (for this may or may not be present in a Will-act) they are conated thoughts and actions.

III. *The Concept of Conation and its Applications.*

What is Conation, if we are in any way to include Will-processes under this head or connect Will and Conation? And what is the origin of our concept of Conation? Literally, Conation is a striving; the notion of it is an abstract notion, and therefore to be accepted with caution. Conation is the actual striving, or inclination, or tendency (that is, the potential striving) of some thing that strives, tends or inclines towards, or away from, some event. And the event is itself but an aspect of the conating thing. Conation is thinkable apart, as is 'force' or 'function'; but it is not therefore *thought as* existing apart from the being which tends and the event towards which the tendency is directed.

This notion of conating, striving, tending, inclining, we apply not only to living organisms, but even, by an extension of analogy, to inanimate beings as well. We not only conceive of animals striving, no matter how blindly, towards goals mentally presented in some way analogous to that in which *we* strive towards goals; we not only think of plants as tending towards the realization of ends, of which we can scarcely credit them with 'consciousness' at all; but we even conceive of inorganic elements and compounds also as tending towards combinations with others, upon much the same pattern[1].

That such conceptions exist is indubitable, no matter how the method and terminology of science have tended to refine them away. The teleological view, the view which anciently made of ends true causes, which has recently been so uncompromisingly re-stated by McDougall[2] in his insistence upon 'purpose' as a necessary category in psychology, and our natural tendency to apply it, cannot be gainsaid. Yet we have no possible direct access to the facts involved in our conception, thus broadly considered. Whence, then, does it arise? What is its origin; and how can it possibly be framed?

IV. *The Origin of the Concept of Conation in Experience.*

(*a*) *Theoretical.*

I lay it down as an evident principle that all concepts (as, *e.g.* of the ether, force, or God) are in some way derived from immediate experience. To say that Köhler's chimpanzees acted teleologically in their attempts to reach the fruit that lay beyond their grasp, and laid their sagacity tribute to their conation towards that result; or to say that action towards an end is to be observed in the nest-building, food-supplying, egg-laying activities of solitary wasps; or in the growth and fructifying of a plant; or even in the actions and reactions of chemical elements, is to say that we are interpreting behaviour (in its widest sense) by analogy with a relation immediately and insightfully cognized within experience.

---

[1] Cf., in this connection, the extension of 'memory' or mnemic function to plants, and even metals, by authors such as Semon and Piéron.

[2] *An Outline of Psychology* (Methuen, London, 1923).

If we seek, then, within experience itself for a relation of this sort, we shall find it only in a relation, apprehended not abstractly, but *together with* the terms of which it is the relation. For the general notion of Conation is that of the striving of a being—the tending towards, or averting itself from—an event or end.

The sense of effort, which has been reported in all the experimental work upon Volition, *taken by itself*, is therefore not capable of being the experimental basis of the notion in question. Nor is the consciousness of act, or of action, signalled as introspectively distinct from that of effort by investigators of Will-process: and this for the same reason. The consciousness of a relation alone, or of a fundament of a relation alone, cannot account for the notion as we undoubtedly apply it in reading teleology into Nature. Our reading of Nature may, indeed, be wrong; but that has nothing to do with the present consideration.

There remains as the basic fact from which the teleological concept can be derived the immediate experience of one thing conating, or tending towards or away from, another thing, or end, or event. This alone fulfils the requirements. But we, admittedly, have no such experience with regard to animals, or plants, or minerals. The only possible experience of this sort must be—and here, for the moment, I argue hypothetically—an experience of the Self as one term in conative relation to another. Theoretically, I submit, such an experience is *necessary* if we are to be able to account for the notion of Conation at all, or to apply it in the teleological view of the world. It is also *sufficient*; as can be shown by the application of the principle of correlate-eduction as formulated by Spearman[1].

(*b*) *Evidential.*

Logical necessity and sufficiency, however, are no guarantees of fact or occurrence. Psychological evidence is necessary for this; and psychological evidence is forthcoming.

---

[1] *The Nature of Intelligence and the Principles of Cognition*, p. 91 (Macmillan, London, 1923).

I have already[1] brought forward evidence to show that it was precisely this Self-doing-something that was experienced introspectively, and especially reported in the protocols of experiments upon Will-acts.

The 'act' and the 'action' of which consciousness had previously been reported is no consciousness of *mere* relation, but the full-blooded, if elusive, consciousness of the Self-acting-in-some-determinate-way. In this consciousness two terms, or fundaments, and a causal relation are insightfully cognized. Whether, *de facto*, the relation is really a causal one transcendentally to experience does not here interest us. The experience in question is necessary and sufficient to permit of its being applied to other cases in an extension of the teleological conception.

I submit that this experience both justifies in principle, and explains the genesis of, the conception of the relation of Conation as generally applied to fundaments other than those of immediate experience.

But there is more in experience than merely the Self, the end brought about by its act, and the relation between them. The experience is not a mere abstract awareness of fundaments in relation, but of a process in which the relation as well as the fundaments are undergoing change. And in that process we are aware also of a modification in consciousness from tension to relief; of a conflict, or discord, which comes to be resolved in a harmony.

All experienced striving, as a process which attains its end, is of this character. This is experimentally evidenced (most clearly, perhaps) in researches which have been carried out on Voluntary Choice. In these researches tension is provoked by the very conditions of the experiment. The Subject is placed in the necessity of having to determine between two alternatives presented to him. Here he observes striving, activity, tension, coming to an end in what is—at least relatively to the process in question—invariably relaxation and rest. But this end-state of the 'choice' is in reality the end-state of the process of the organism choosing; and it is apprehended as a state of well-being of the organism as opposed to one of inner conflict. It is characterized by Subjects in such terms as 'relief,' 'satisfaction,' and the like; and, where hedonically toned, is pleasurable. In other words, the end-state of 'choice' is relative perfection as contrasted with imperfection.

V. *General Principle of Conation.*

This character of transition in 'choice' from tension, conflict and imperfection to relaxation, well-being and perfection[2] is important; for it enables us to formulate a fundamental principle, rooted in direct experience, of all Conation. What is our concept of Conation (the analogue) which must be paralleled by experiential cognition (the type)? It is this, stated as a principle, the scope of which might, of course, be extended:

---

[1] This *Journal*, January, 1926, xvi, pt. 3, 159.

[2] Quite characteristic of processes of choice.

Every living organism tends, within the limits of its life-cycle, towards preserving its own integrity; i.e. *attaining its own perfection*[1].

This is not a psychological principle; it is a biological one[2]. Nevertheless it could not even be formulated were it not in some way given as a concrete instance—or indicated *in* a concrete instance—including fundaments, relation and transition, in immediate experience.

(I have added, in qualification of the principle stated, as generalized from direct experience, the words "within the limits of its life-cycle." I do not propose here to enter in detail into the reasons why such qualification must be made. But it will be clear that, in the view I advance, evidence for it must be derived ultimately from immediate experience also. No doubt a long process of building up relations one upon another, and of evolving correlated fundaments, is involved in its formulation; but complicated mental processes of this kind are not unusual; and the ultimate result may be reached in this, as in them, with insight.)

The principle just enunciated is to be considered as the broadest generalization possible in so far as living organisms are concerned, and as fundamental to all more narrow statements of the same, or similar, tendencies. For, within the scope of this absolutely general tendency, there are others of a more specific kind. There are strivings, or tendencies, towards specific 'ends,' as exhibited in tropisms and in instinctive actions, in which the organism is concerned. These are manifested within limits in a more or less definite way, as regards the character and order of the actions involved, when an appropriate stimulus, or situation, or seasonal incitation, or the like, acts upon it. No doubt these may be considered to be partial determinations of the organism which are integrated in the general tendency of self-conservation, maintenance of integrity and teleological balance. Or, perhaps more truly, such conative tendencies as are exhibited in the working of tropisms and instincts may be considered as the fundamental hormic forces which, *as* integrated, constitute the general tendency formulated in the principle.

---

[1] Cf. Haldane, "Psychology and Biology," in *Psychology and the Sciences*, edited by William Brown (Black, London, 1924).

[2] Cf. Walston, *Harmonism and Conscious Evolution* (John Murray, London, 1922). Cf. also this *Journal*, 1923, XIV, pt. 2, 199.

In any case, we ourselves are capable of having that direct experience of object, impulse and emotion involved in the working of an instinct, which enables us to generalize in the manner indicated.

VI. *Principle of Instinctive Conation.*

At this level—since consciousness seems plainly to enter into instinctive action—we are justified in explicitly formulating a psychological principle. Whereas the more general tendency of preserving and recovering organic integrity—as instanced in the physiological processes subserving the healing of wounds or fractures, general adaptation to environmental changes, and the like—may be formulated without explicit reference to psychology; instinctive tendencies cannot be explained so easily in this way. We may therefore attempt to state a principle of Conation on the instinctive level as follows:

All living organisms evolved to the perceptual level (or level of sensory apprehension) tend to strive in a more or less definite and predetermined manner towards specific ends, or goals of action, when excited by the presentation of a stimulus coordinated with such a tendency.

No doubt the significance of instinctive action is biological; but there is no less clearly a psychological significance as well. And this latter significance is amply warranted by our own direct experience of conscious striving towards goals or ends present to consciousness in us and consciously desired. Though we must still work with the tools of analogy, instinctive action appears to conform more nearly to our own direct experience than does the kind of action considered in the first enunciated principle.

We are thus far brought to look upon Conation in the concrete as being the organism itself striving towards ends—or, if not actually striving, at least as connately 'set' to the performance of certain kinds of action, or behaviour, upon appropriate stimulation. Conation is thus a dynamic process (or the potentiality of such a process) in which the behaviour of the organism itself maintains, or brings about, integrity. The living organism, therefore, is itself the efficient cause of such effect. I need not in this paper consider the numerous 'contrary instances,' in which integrity is *not* secured by instinctive action—cases like those of the moth and the candle, or the unique nuptial flight of ephemera, in which Conation issues in greater tension, and, indeed, sometimes destroys the organism itself. As could be easily shown, similar cases occur upon the still higher plane of intelligent choice; and it is possible that these may throw light upon the apparently obscure problem of connate tendencies that issue in the ill- rather than the well-being of the organism possessing them.

So far we have been considering Conation in its conscious aspect; and we have interpreted tropistic and instinctive action by analogy with tendencies, impulses and actions of which we ourselves are, or may be, fully aware. In doing this we have left out of account all consideration of reflexes, of glandular secretions and of those continuous, rhythmic or interrupted, biological processes which are equally with them conative in character. These, however, will readily fit in among the facts expressed in the first principle; if we make allowance for them, or some of them, being only partial tendencies integrated in the more general one. The total conative energy of the organism cannot be considered as normally being exhausted by any one of them singly. Nor, again, can the instinctive actions that take place on the perceptual level be viewed as normally exhausting Conation; though, when they are functioning, they may undoubtedly drain it from other channels in which it usually flows. I am thinking of the disorganization of the digestive functions under the influence of strong instinctive conation; as, *e.g.* in the case of the cat angrily defending itself or its kittens against an aggressive dog; and of the circulatory changes consequent upon Conation[1] as indicated by psychogalvanic phenomena.

VII. *Special Characteristics of Choice.*

We come now to the consideration of an actual exercise of Conation in the case of human voluntary acts, of which we can have direct experiential knowledge. This has been presupposed in the foregoing, as it is on the analogy of the immediate experience of Conation in such acts as those of choice that we have been able to interpret the phenomena already examined. But human choice presents, over and above the common features shared with Conation in general, several important and distinguishing characteristics, which make of it a process *sui generis.* In the first place, choice involves two alternatives, ends or events, at least; and these are presented to consciousness as in some respect objects of *desire*. In general, a principle of conscious desire might be formulated, which in most essential details might be assimilated to the second principle already proposed. Some sort of appetition, together with its feeling tone, may clearly be attributed analogically to instinct. But in what may be called true Volition (using the term in its old and more exact sense) there is certainly contained in the consciousness of desire the essential note 'object-desired-by-me,' or 'event-desired-by-me.' If self-consciousness is to be denied to animals other than the human, it is evident that such an experience must be denied to them also.

---

[1] Cf. this *Journal*, July, 1925, XVI, pt. 1, 50; cf. also, "The Effect of the Circulation on the Electrical Resistance of the Skin," *Journal of Physiology*, September, 1925, LX, no. 4, 316.

But further, in choices there is not merely the consciousness of desire—'Self-desiring-this-object, -end or -event'—but there is also the consciousness of the Self in two possible concrete conative relations: viz. as conatively related to event A, or to event B. This, no doubt, is an awareness of an ideal, rather than an actual relation mediating the fundaments. But it is the kind of awareness that characterizes all choices at some early stage of the period of motivation. The alternatives presented are evolved during this period as values which are always implicitly and very often explicitly[1] related to the Ego, and *are* values only in virtue of such a relation, by which their worthwhileness is constituted.

Moreover, in all the experimental work on Choice with which I am acquainted, the alternatives, though they are ends (*i.e.* though the designation or acceptance of one of them is the event of the choice) are never absolute ends *in* themselves. Each is a consciously possible *alternative* to a further end; and thus neither resembles the definite end of an instinctive or impulsive action which satisfies itself.

In experimental conditions the nature of the choices is necessarily a somewhat artificial one. Tastes, odours, arithmetical operations and the like provide, it is true, alternatives for choosing. But, instead of the naturally arising desires which actuate the discussion of motives in the choices of everyday life, in the laboratory the motives are evolved under the influence of the instruction: "Choose, for a good reason, between the alternatives presented."

This does not invalidate the conclusions drawn from the introspective reports of the Subjects, however; nor does it denaturize the processes of choice observed; since the motive for choosing at all in the circumstances is the wish (*i.e.* desire) to carry out the instruction previously accepted, to discover the phenomena which constitute Choice and so advance scientific knowledge, to become a psychologist and so fit oneself for future work, or the like. Such motives can be reduced to one or other of the instinctive tendencies; and in particular, as a rule, to the self-assertive.

---

[1] This is abundantly clear from introspections.

VIII. *Principle of Volition.*

(*a*) *In 'Desire.'*

What actually happens, then, in choice between alternatives? Is it possible here also to formulate a principle—reconstructing the typical process of choice from a large number of introspections, in any one of which details (and even important details) may fail to be observed[1]?

In general, it may be laid down as a principle that *The human organism tends to strive towards any end desired.* Striving of this sort presupposes previous experience of such end, or a knowledge of it reached by way of the principle of Correlate-eduction; and such Conation as is involved in the process differs from that exhibited in instinctive reactions precisely in this, that it is a conation guided by conscious reinstatement, or mental fabrication, of an end, together with a tendency towards it. In either case, however, the determination of the organism to the end desired consists radically in a connate relation between the organism *and* the end, as is also the case in instinctive actions.

(*b*) *In 'Choice.'*

But in choice there are *two* alternatives or more, several immediate ends of action desired, which are involved. And here the principle must read:

"*The human organism tends to delay action provoked by stimuli or situations which arouse desire, when and in so far as it is able to apprehend the relations of several possible means (more or less nearly balanced) to a further end desired.*"

The actual processes observed in experimental conditions support this formulation. In the first place, there is the end desired which is created by the accepting, on the part of the Subject, of the instruction. This, as is well known, may be clearly present to consciousness; or may, on the other hand, have sunk to the unconscious level of a 'mental set.' In either case the whole reaction of choice is determined by it; and cannot take place without it, or its equivalent.

Then comes the presentation of the alternatives to consciousness, and the period of motivation. One discovers every phase, from impulsive adhesion to one of the alternatives (which can scarcely be considered to be a true choice at all) to a complete inability to determine the process of the evolution of motives by actually choosing. Between these two extreme cases, actually observed in experimental work, there is found every variety of hard or easy choice. And, in every case, the end is desired, the alternatives are apprehended, as well (at any rate until the choice experiments become more or less habitual) as the relation of the alternatives as means to that end. One cannot add or subtract, he cannot drink one of two liquids, until he has made up his mind what he will do. In other words, in every case an alternative must be selected. This is all fairly obvious; but introspective analysis enables us to unravel in greater detail the process of Choice. Delay in action is due to the deliberation of motivation. Choice depends upon a judgment, or quasi-judgment, in which the process of motivation comes to its end; and it ends in the acceptance of a value by the Subject. Further, the value itself is determined by the relation of the alternatives to the Subject in the conditions of the experiment. And, finally, this relation—we may here be permitted to speculate—can only be one itself determined by the Subject's dispositions, or connate and acquired tendencies, towards the several alternatives as means to an end.

---

[1] Cf. the principle of Mental Energy, by which this fact may be explained. Spearman, *The Nature of Intelligence and the Principles of Cognition*, p. 131.

IX. *Relation of Value to Motive.*

It would be desirable further to be able to enunciate as a principle the relation of value to motive; and to say that: "*That value is always determined as a motive which best accords with the disposition of the Subject; it is made his own motive in accordance with that disposition.*"

But that would be to go beyond the psychological evidence at present available; and, indeed, it is difficult to see how empirical evidence for it could be adduced.

In any event, the case is not necessarily so simple as suggested. It may be enormously complex. There is a tendency, for instance, to choose the easier arithmetical alternative, or pleasant, rather than unpleasant, taste. Yet the more difficult or unpleasant are sometimes actually chosen for 'ethical' or other reasons. That is to say, other tendencies or dispositions may complicate the (presumably) primary one involved.

This consideration brings us to a form of Choice in which not one, but several tendencies, are stimulated. Again, we must confess to speculation rather than empiricism; but we might hazard a further formula here:

That value is always determined as motive which best accords with the totality of the dispositions involved in the Choice: i.e. *the dispositions which are integrated as 'character.'*

And this, I suppose, would be the ordinary case of everyday life.

We have empiric evidence, however, that such dispositions are connate or acquired. The connate dispositions are instinctive in character (impulsive and determined towards the ends of their activities). The acquired dispositions are built up upon them. The fact that, when such dispositions become actual conations, they issue in action *which relieves tension*, points to the explanation that they tend always to restore mental balance and equilibrium or integrity. The concept derived from this experiential fact is the one that has been employed throughout in this paper.

X. *Distinction between Conation and Volition.*

Up to this point, though we have been analysing the process of voluntary choice, we have been mainly concerned with Conation. The actions, physical or mental, which relieve mental tension are conative actions, as are the tendencies released by the choice. It is the passage from mere tendency to actual conation, I am convinced, that is translated into consciousness by the phenomenon reported by various investigators as consciousness of act, consciousness of action, alertness, and the like. This has been shown by Michotte and others to be a consciousness quite distinct from that of the sense of effort. Evidence has elsewhere[1] been adduced that the degree of such conation can be estimated with some accuracy by Subjects, and that the estimates are objectively corroborated by physical measurements. But if this belief is founded—if awareness of act, action or alertness is awareness of Conation—how does Volition enter into choice, and in what way is it present to consciousness?

The greater part of the complete process observed, from the exhibition of the alternatives to the final reaction, is of a predominantly cognitive character. Perception of the alternatives, elaboration and evaluation of motives, is clearly of this nature. But then follows, most consistently, what has been described as an alteration of level of consciousness, a complete change, an arrest, and the like. The predominantly cognitive character of the process changes to a predominantly conative one; and reaction ensues. Apparently the only clue we have to the intervention of Volition lies in the consciousness of Self, the evidence for which was developed in a previous paper in this *Journal*[2].

In previous work[3] Michotte related consciousness of action to the Self as its phenomenal presence to consciousness, and he connected it with voluntary activity, while asserting that it was also to be found in processes other than those of choice which he investigated. Ach maintains[1] much the same doctrine. And we agree with these authors. But, it would appear, if our analysis and interpretation of the phenomena are correct, that the true Will-act does not manifest itself to consciousness in alertness or consciousness of action, but that, in certain circumstances, it is a condition of the release of Conation which is thus phenomenally present.

---

[1] Cf. Wells, this *Journal*, January, 1924, xiv, pt. 3, 300.

[2] This *Journal*, January, 1926, xvi, pt. 3, 159.

[3] *Étude expérimentale sur le choix volontaire*, Louvain, 1910.

There is evidence in the researches upon Choice that the circumstances in which the release of Conation is a voluntary one in the exact sense of the term consist in the adoption by the Self, the identification *with* the Self, of the motive or motives for the selection of one of the alternatives. Such adoptions, identifications, strengthenings of motives by reference to the Self, and the like—in short, such conversions of values into effectual motives *by* the Self—are frequently reported in the introspections. Indeed, the evidence is all in favour of the general statement that motives in the abstract are not motives at all; they are only motives because they are mine, or yours. And this appears to be in reality what James's 'fiat' stands for.

If this be true, as I submit the evidence leads us to believe, we may generalize still further and assert that all voluntary action is of this nature: viz. the identification of a value, as a motive, with the Self.

XI. *The Intuited and the Conceived Self.*

But *what* Self? For we have already distinguished[2] between the Self as intuited or immediately given in notional experience, and the Self as conceived. Undoubtedly the profound Self as conceived by us, one aspect only of which is given in the immediate experience. The Self which I experience meagrely and barely, now as perceiving, now as judging, now as willing or choosing, is a Self conceived by me as a Self rich in possible modes of activity, possessing already many determined values, social, ethical, religious, and the like; which are of the general nature of ideals. All these—the conceptual Self, full of content and reality, the already formed ideals, together with those in process of formation—though they are conceptual, are themselves grounded, as I have elsewhere[3] attempted to show, in immediate awareness.

And it is to this intimate personal complex, of which now one and now another aspect is contemplated, that the values which may, and do become motives are referred. It is this Self that makes the motives its own, while revealing itself to introspection as a Self almost empty of content, which here and now merely chooses.

---

[1] *Über den Willensakt und das Temperament*, Leipzig, 1910.

[2] *Loc. cit.*

[3] *Loc. cit.*

This apparent antinomy between the concept Self and the intuited Self is in reality nothing new or strange. An absolutely similar situation is presented in objective perception. Here also the actually given sense-data may be extremely meagre; but the completed percept is full and rich, though nothing save a knowledge that there *is* far more than the presented items may be given on introspection. And the more energetically one concentrates upon part of the presented data the less do the remaining parts intrude themselves upon awareness. Reasons were given in the previous paper why the direct consciousness of Self should be so meagre and empty of content; and they are paralleled by those, summed up in the law of the distribution of cognitive energy, that explain these facts of objective perception also.

XII. *The Concept of Freedom.*

This brings us to a final point of very great importance. Having thus distinguished Conation in general from that particular form of activity which we know as Volition, can we formulate any principle in regard to this latter?

The Volition involved in processes of Choice—and, indeed, it would seem any Volition whatsoever—seems to be so capricious, so intensely independent of determination, that it must escape all law. This, at any rate, is the popular belief; and this, no matter what our scientific postulates or philosophical views may be, is the pragmatic attitude of every one of us.

We are in presence of the problem of 'freedom.' Now, as a general rule, the psychologist shirks this problem, handing it over to the philosophers as belonging in no sense to his science, and refusing to admit that there is a very real sense in which it belongs peculiarly to psychology. We have no right to shut our eyes to difficult problems, or to avoid them because they happen to be highly controversial. It is incontrovertible that the notions of freedom and determinism exist; and it is our plain duty to account for their origin. We may be led to the position of asserting that there is no law, in the scientific sense, which we can formulate. But, if we pursue the introspective evidence—as we have every right to pursue it—to the ultimate conclusions to which we are led by the apprehension of relations and of correlates, we shall find that there is an explanation of the origin of these notions, and that we are able to formulate a principle, at least in the philosophical sense, in their regard.

The crude data from which we set out in the psychological investigation of this problem are these. There are actions and trains of thought in which we feel that we are constrained; and there are others with regard to which we feel ourselves to be free. These are the experiential data from which the notions of freedom and determinism are derived. We are not for the moment concerned with the validity of these notions as applied to any concrete entity or event, but only with their genesis. Now, in virtue of the evident principle which has been employed throughout in this paper—viz. that all concepts are in some way derived from immediate experience—the mere fact that we believe we are not always constrained (in Choice, for example) to choose one rather than the other alternative, points to some differentiation in the phenomenal consciousness between the two types of experience. The evidence to be gathered from the introspective reports of Subjects in this matter is closely connected with the experienced Self in its relation to the alternative chosen. The difference does not lie in the evolution of the motives (except in so far as this is delayed until motivation is complete), for this is cognized as cognitive in character. Nor does it lie in the motives themselves as finally elaborated, for these, too, on their subjective side are processes of cognition. Nor, if we are right in ascribing the phenomenon to Conation, is the difference to be found in consciousness of action, taken with or without a further consciousness of effort. It lies uniquely in the awareness of the Self-as-choosing: *i.e.* as actively accepting one of the alternatives, designating it, *making it its own*; in short, in identifying it with itself. It is submitted that this *is* the consciousness of 'free choice.' Given with insight the fundaments and relation 'Conceived-Self-making-Motive,' we can account for the concept of free causality; and the notion of freedom is thus explained in its origin. From this, whether it hold true metaphysically or not—*i.e.* whether *de facto* man is or is not a free being—is derived our psychological notion of freedom; just as from impeded or non-effectual desire is derived our notion of constraint. But impeded or non-effectual desire is not so much a volitional as a conational experience. We can will perfectly well even if we cannot carry our willing out in action. Thus the complex notion of law in the scientific meaning of the term, connoting, as it does, determinism, is posterior to that of freedom; for it arises as a result of impeded conation resulting upon Will-acts. And this is borne out by the witness of genetic psychology, both in the case of primitive peoples and in that of the child. 'Freedom' appears to be a concept formed anteriorly to that of 'determinism.'

PERSONALITY AND VOCATION

A NOTE ON EFFECTS OF INTROVERSION ON DOMINANT INTERESTS

By W. V. BINGHAM.

Personnel Research Federation, New York.

I. *The Concept of Introversion* (pp. 354, 355).
II. *Ways of Measuring Introversion and related Aspects of Personality* (pp. 355–357).
III. *Relationship between Introversion and Vocation* (pp. 357–359).
IV. *How Introversion Modifies Dominant Interests* (pp. 359–361).
V. *Implications and Conclusions* (pp. 361, 362).

I. *The Concept of Introversion.*

INTROVERSION means the tendency of interest to shift from the object to the subject. Chiefly in evidence in social situations, it is marked by heightened self-consciousness. A behaviouristic definition of introversion stresses exaggerated tendencies to delay response, to inhibit overt emotional expression, to withdraw from social contacts. In these respects, one thinks of personalities like Hamlet, Poe, Royce, Steinmetz, and Woodrow Wilson as similar. They all tend to substitute feeling or meditation for prompt action.

This is not the place to evaluate proposals for distinguishing different types of introverts such as the reflective and the affective, nor to discuss those ingenious physiological theories which find a vehicle for introverted emotional energy in the autonomic system, and assume that the overt, immediate emotional expression of the extrovert is adequately taken care of through the cerebro-spinal system and the skeletal musculature. Neither shall we debate as to whether people can in general be classified as belonging to two contrasted types. We shall on the contrary assume that the distribution is unimodal—that the most frequent personality is well balanced or ambivert, and that introversion and extroversion represent tendencies, more or less extreme, to deviate from this mean. Such tendencies are determined only in part by inborn reactive equipment, glandular regulatory mechanism and the other inherited physiological bases of temperament; for introversion varies with growth, training and social circumstances. Youths in early and middle adolescence, for instance, show more tendencies in this direction than either younger boys or college men. Introversion is subject to extensive modification through experience and training, beginning with infancy. Moreover a person may exhibit marked fluctuations from introversion to extroversion and back again, if his circumstances change radically from year to year. Periods of success, prosperity and personal satisfaction alternating with periods of failure, disappointment and chagrin produce in some personalities clear-cut shifts from extroverted behaviour to its opposite, reminding one in a mild way of the contrasted phases of the manic-depressive psychosis. When the extroverted Mr Babbitt attends a religious revival and comes for a time under conviction of sin, he becomes decidedly introverted until conversion and confession of faith once more establish his self-regard. But in spite of such oscillations, we nevertheless recognize that many personalities remain more or less consistently extroverted, or introverted.

We shall examine the relation which investigators have found to exist between introversion and certain vocational interests, asking what these relations are, how they are to be explained, and what implications, if any, may be gleaned from them for vocational decisions. But first we shall refer briefly to some of the investigations which have led to the formulation of these questions.

II. *Ways of Measuring Introversion and Related Aspects of Personality.*

Current research on the measurement of non-intellectual aspects of character and personality need not here be summarized in any detail. Thanks to May and Hartshorne[1] digests of a large portion of this rapidly accumulating literature have recently been made available.

An outstanding aim of recent research is to escape from the uncertainties of subjective estimates of traits by presenting controlled situations in which characteristic behaviour is given a real chance to operate. The psychological test and the school examination lend themselves to such purposes. In the behaviour of a person confronted by a set task, varied aspects of personality and character come to light, and offer themselves for quantitative study. Dr Manson[1], for example, has compared vocational groups by noting measurable differences of behaviour under such standardized conditions of instruction and incentive. In the mental alertness test known as Bureau Test VI (consisting of Army Alpha items arranged in spiral omnibus form for convenience in administering), the instructions to subjects are, not to omit any items. But comparison of various occupational groups according to proportion of items actually omitted under these uniform instructions, reveals significant differences. Two of these groups consisted of women college graduates. One group was at the time taking professional training for educational work. The members of the other group were preparing for business positions. On examination, the two groups were found to have practically the same average score in intelligence, but the business group omitted more items than the academic group. Those who wish may speculate as to the proper designation of this difference. It matters little whether we say that the academic group exhibited more caution or conscientiousness, or that the business group excelled in initiative. Whatever the name, there is no doubt as to the objective fact of a significant measured difference in behaviour between these groups with different vocational bias. Such measured by-products of intelligence tests give promise ultimately of yielding information scarcely less valuable for individual appraisal than the intelligence scores.

---

[1] Mark A. May, "The Present Status of the Will-Temperament Tests," *J. of Appl. Psychol.* 1925, IX, 29–52; Mark A. May and Hugh Hartshorne, "Objective Methods of Measuring Character," *Ped. Sem.* 1925, XXXII, 45–67.

Among techniques for measurement of non-intellectual traits, the word-association reaction continues to be serviceable. It furnishes, for example, a measure of commonality of response; and the proportion of unique or individual word-associations is an indicator of the subject's tendency to let his thoughts revolve about his own personal experiences. Here then is one rough measure of introversion. Other useful indexes are, small proportion of 'opposite' responses, and of 'commonest' responses. These ratios are simpler to determine than the index of commonality computed from tables of frequency after the method of Kent and Rosanoff. Dr Marston[2] has recently reported new experimental methods for objective measurement of degree of social resistance, compliance with another's will, caution in making decisions, interest in environment, and self-assertion, but has had recourse also to rating methods in his attempt to get a quantitative measure of introversion. Cross-out tests, interest questionnaires and similar measures of likes and dislikes, continue to have wide usefulness in furnishing clues to personality trends. The Downey Will-Temperament tests have been found repeatedly to yield measures of low reliability, which properly discourages their use in practical individual diagnosis; but parts of this series have furnished measures of difference between vocational groups with an index of significance which the research worker cannot afford to ignore. These clues must be followed until dependable measures of affective and conative characteristics have been brought to a stage of development corresponding to the standardized examinations of intelligence and of knowledge.

---

[1] Grace Manson, "Personality Differences in Intelligence Test Performance. Group Individual Differences: The Significance of 'Omissions,'" *J. of Appl. Psychol.* 1925, IX, 230–255.

[2] Leslie R. Marston, "The Emotions of Young Children—An Experimental Study in Introversion and Extroversion," *Univ. of Iowa Studies in Child Welfare*, 1925, III, no. 3, 1–100.

III. *Relationship between Introversion and Vocation.*

It has been found that persons with tendencies toward introversion are relatively more frequent in vocations requiring mainly ability to deal with concrete objects or mechanisms, with ideas, with symbols, written words, numbers, diagrams or other abstractions. People engaged in the work of accountant, statistician, inventor, engineer, mechanic or poet are often somewhat introverted. But where success demands primarily efficiency in social contacts, as in military command, politics, factory management, office supervision or selling, extroverted personalities are relatively more common. The scholar often belongs among the introverts. The successful business man more frequently shows a tendency to extroversion.

In illustration of studies which furnish the ground for this summary regarding personality and vocation, reference may be made first to the investigations of Dr Freyd[1]. It was in 1922 that Freyd made his very significant measurements of differences in personality of two contrasted occupational groups. One group of his subjects consisted of 127 students of technology pursuing courses in industrial or mechanical engineering. He assumed that in choosing such a career they had expressed an inclination of interest in the direction of dealing with mechanisms. In his other group were 493 salesmen or students of life-insurance salesmanship. Membership in this group signified, in the investigator's opinion, an expression of interest in dealing with people. While no doubt the difference of fundamental interests between these groups is not to be sharply drawn, it is reasonable to assume that in the group of technology students there was a preponderance of mechanically inclined persons, and in the other groups a preponderance of socially inclined. The members of both groups were measured by means of preference questionnaires, rating scales and psychological tests. Group comparisons were made. At the close of the investigation it was possible to arrive at the following statement of significant group differences between the students of technology and the salesmen, between the mechanically and the socially inclined:

---

[1] Max Freyd, "The Personalities of the Socially and the Mechanically Inclined," *Psychol. Monogr.* 1924, XXXIII, no. 4, whole no. 151.

> The results of the group comparisons lead to the conclusion that those who are interested in dealing with human beings (the salesmen) are differentiated from the students of mechanics by greater social ability, credulity or suggestibility, adaptability, excitability, self-confidence, talkativeness, present-mindedness, and good nature. The students of mechanics show a greater tendency to be self-conscious, careful of details, inhibited and cautious, reticent, absent-minded, and glum. They are more capable of making fine co-ordinations and excel in slow, painstaking, and accurate effort[1].

No one familiar with Jung's concepts of introversion and extroversion can fail to be impressed, as Freyd was, with the close similarity between the usual clinical descriptions of these contrasted types of personality and the experimental findings with reference to the characteristics of the mechanically and the socially inclined. The description of the typical student of technology turns out to be a pretty fair picture of the typical introvert. The typical student of life insurance salesmanship, on the other hand, more closely resembles the extrovert[2].

The significance of these measures of differences between the socially and the mechanically inclined has been re-examined and corroborated by Miss Hubbard[3], who reports in the *Journal of Personnel Research* for January, 1926, a study of 1097 students in colleges and professional schools of the University of Minnesota. She finds that groups of university students who have elected engineering as their profession make significant negative scores on Freyd's scale of social interests. Groups of law students are at the opposite extreme, while in between are the students in the liberal arts courses. Professor E. K. Strong, Jr., Dr Karl M. Cowdery and their associates at Stanford University, are finding, by an adaptation of this interest analysis procedure, a striking differentiation between students of law and other professional groups.

---

[1] *Op. cit.* v.

[2] Max Freyd, "Introverts and Extroverts," *Psychol. Rev.* 1924, xxxi, 74–87.

[3] Ruth M. Hubbard, "Interests Studied Quantitatively: Measurement of Differences between the Socially and Mechanically Inclined in Relation to Vocational Selection," *J. of Personnel Research*, 1926, iv, 365–378.

It has often proved useful to differentiate people with reference to their relative interest in manipulating mechanisms, motivating people, and dealing with symbols and ideas. In the first of these groups, the mechanically inclined, we have seen that there is an excess of introverted personalities, while extroverts appear in larger proportion among the socially inclined. Are the facts equally clear with reference to the third group, made up of people with a strong bent for intellectual work, study, research, and writing? No accumulation of experimental evidence is at hand; but the observation will probably not be seriously challenged that scholars, whose vocation it is to deal primarily with concepts, exhibit tendencies toward introversion more frequently than toward extroversion. That executives in business and industry show clearly the opposite tendency is evidenced by repeated studies made by Dr C. S. Yoakum and his associates in the Bureau of Personnel Research between 1921 and 1924. These results are now confirmed by Johnson O'Connor of the General Electric Company, in an investigation shortly to be published.

IV. *How Introversion Modifies Dominant Interests.*

What is the ground for this connection between mechanical interest or intellectual interest and tendencies to introversion of personality? Why are engineers and scholars more apt than salesmen to be living an inner life of reverie? Which is cause and which effect?

The theory here advanced is, that early introversion of personality leads to the development, *through disproportionate exercise*, of one's native interests in mechanism or ideas, at the expense of interest and proficiency in social contacts.

The boy with little more than average native mechanical ability but with an exaggerated tendency to shrink from his social environment, spends more time playing with mechanical contrivances. His interest in mechanism grows apace with increase in his mechanical knowledge and skill. He eventually develops a marked vocational tendency which would have been less in evidence had he been less prone to introversion. His ultimate mechanical ability is to that extent a function of his social personality trends.

Assume two brothers of equal natural aptitude for dealing with machines, but differing in personality. Childhood experiences coupled perhaps with a native bent have given one of them a tendency to introversion. He shrinks from social contacts. He escapes from them and, with nothing else to do, buries himself in his plays of mechanical manipulation. Whatever original talent he possessed for managing machines is enhanced by practice. Interest grows with skill. He becomes relatively expert in a direction his extroverted brother might also have followed, so far as specific native endowment goes. The bending of the twig toward introversion inclines the tree towards the mechanical. The young man becomes an inventor or an engineer, instead of an actor, a salesman, or a business executive. His ultimate mechanical ability has been in part conditioned by his personality.

So too with the scholar. He may be equipped by nature with very exceptional capacities for manipulating concepts, or his intellectual endowment and interests may at first not be so outstanding. If, in either case, his personality as child or youth becomes somewhat introverted, he tends—more than his fellows with equal mental capacity—to devote himself to reading and study. He gets the habit of living in his library or his laboratory. Here he develops an intellectual competence and a persistent interest in scholarship which he would hardly have acquired, lacking a bent toward introversion. Eminent achievement in scholarship and research, no less than in engineering or mechanical pursuits, may be fostered by such early trends of personality.

Both interest and ability are functions of personality. Each affects and modifies the other. Vocational ability grows with exercise and training. Interest in any type of activity usually increases with growth of pertinent knowledge and skill. A bent toward introversion, then, is favourable to the enhancement of both interest and ability in dealing with machines, while the opposite sort of personality favours development in social interests and social effectiveness. Such considerations make plausible the facts—at first glance somewhat astonishing—regarding the correspondence found between introversion and vocational interest in mechanisms.

Some of our British colleagues have insisted that in forecasting vocational ability, no dependence can be placed on boyhood interests. It may well be that the lad of twelve or sixteen has had too circumscribed an experience to give much validity to his expressions of preference. But certainly in later youth and early manhood, during and after college years, the correlation between interest and vocational ability is positive, and so large that a careful measure of occupational preferences yields in some instances the most valid single prediction of vocational aptitude. Typical of such results is Dr B. V. Moore's study of college graduates in training for positions as machine designers, production managers and salesmen, for the Westinghouse Electric and Manufacturing Company[1].

V. *Implications and Conclusions.*

Vocational counsellors have always recognized the share which personality contributes toward success or failure in some careers. They know that a young man's bent toward an occupation is most certainly a function of his characteristic emotional tendencies, and of his personality as a whole, as well as of component abilities—general and special—and of external considerations such as opportunities and social pressure. Some of these advisers of youth will instantly appropriate new and promising facilities for measuring personality. But the wiser ones will utilize such data with great caution. And caution is needed, for two reasons: the low reliability of the individual measures, and the great flexibility or modifiability of many personalities. Temperament and habitual emotional attitude differ in these regards from intelligence, which remains roughly constant with the passage of the years. The introvert cannot greatly alter his I.Q., but he sometimes outgrows his shyness, his suppressions, his tendencies to reverie, his slowness to act. The extrovert may learn to meditate, to be more considerate, to check a natural abruptness to action. Vocational interests, long stunted or suppressed, may then be brought to full development.

Measurements of personality traits will not find their greatest usefulness in any programme of guidance which merely aims to match personality with vocation. But they may be valuable as aids toward personality development, first as means by which to diagnose and focus attention on defects to be eliminated, and then as spurs to effort in the course of personality training.

In conclusion: we have offered an explanation of the connection between certain sorts of vocational ability and tendencies of personality. We have called attention to the effects of exercise in developing the relative strength of interests and abilities in dealing with mechanisms, with ideas and with people. Whatever the native endowment in these regards, early introversion tends to develop the interests in mechanisms or in ideas, through giving them exercise at the expense of activities that foster interest and proficiency in social contacts.

---

[1] B. V. Moore, *Personnel Selection of Graduate Engineers:* The Differentiation of Apprentice Engineers for Training as Salesmen, Designers, and Executives of Production, *Psychol. Monogr.* 1921, xxx, no. 5, whole no. 138. See especially chap. VIII, "Survey and Evaluation of Interests as Criteria for Vocational Placement."

We have recognized the importance of personality traits for success in many occupations and careers, and noted that measures or indicators of these essential traits are in process of development. The invention, standardization and validation of these techniques constitute one of the most significant movements in psychology to-day. But for the present, any gesture in the direction of practical utilization of these measures of personality as aids to vocational decisions should be made with the utmost hesitancy, in view not only of their necessarily low reliability but also of the instability of the very personality characteristics whose share in vocational success is obvious. The best usefulness of these measures will always be as indicators of need for personality training rather than as devices for facilitating immediate vocational decisions.

(*Manuscript received* 19 *January*, 1926.)

OBITUARY

PROF. JOSEPH BROUGH.

BY the death of Professor BROUGH on December 7th last Psychology lost a faithful exponent.

As a young man Dr Brough was articled to a firm of solicitors and was admitted in 1875. There seemed every prospect that he would practise his profession with distinction, but his interest in the philosophical aspect of law led him to prefer an academic career and he entered Downing College, Cambridge. In 1880 he took a first class in the Moral Sciences Tripos and in 1881 a first in the Law Tripos. In 1883 he was appointed professor of philosophy at the University College of Wales, Aberystwyth. He had been one of the late Professor Ward's earliest pupils and was much interested in the experimental methods of psychology. He was a pioneer in that he introduced experimental demonstrations into his lectures on psychology as part of the course for a philosophical degree. Such demonstrations had already an established place in his teaching during the early nineties of the last century. He had a profound belief in the value of logic and psychology for mental training. This belief found expression in *The Study of Mental Science* published in 1903. It also found practical expression in the curriculum of the new University of Wales on its foundation. Owing very largely to Professor Brough's advocacy and to his active canvass of expert opinion, elementary logic was prescribed as an essential course for all candidates preparing for a degree. Later he secured a place for experimental psychology among the optional subjects allowed for the philosophical degree. Before his retirement in 1911 he had succeeded in getting together the material and apparatus necessary for laboratory courses in this subject. As recently as 1923 he reaffirmed his views on the value of these courses in a University curriculum in an article, "The Vogue of Psychology" published in *The Welsh Outlook*. When in London he was a frequent attendant at the meetings of the British Psychological Society of which he was an original member.

His intellectual insight and ready grasp of the essential features of a problem made him a stimulating teacher, while the kindliness and sincerity of his character won Dr Brough the warm affection of his students. The fortitude with which he met the calamity of blindness, which first began to threaten him in 1920, well bore out his philosophy of life. Despite the deprivation of his greatest sources of enjoyment—reading, playing the violin, and taking long solitary walks with an archaeological objective—he retained his interests and never lost his delicate sense of humour.

In addition to *The Study of Mental Science* he published articles and reviews in *The Encyclopaedia of Religion and Ethics*, *The Dictionary of Education*, *The Proceedings of the Aristotelian Society*, *The Academy*, and *Mind*.

B. E.

COMMITTEE FOR RESEARCH IN EDUCATION.

With a view to facilitating the general co-ordination of educational research, the Committee has made its annual enquiry as to researches in progress (or planned) at the various University Psychological Laboratories and Education Departments. The following is a summary of the replies received:

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER, 1925.

DEPARTMENT OF EDUCATION, THE UNIVERSITY, LEEDS.

1. Methods of learning some Rhythmical Tasks.
2. The Teaching of Urdu.
3. The Psychogalvanic Reflex.
4. A Study of Transfer of Scientific Method.
5. Appreciation of History, at ages 11 to 15 years.
6. Psychological Tests of Ability in Mathematics.
7. ,, ,, ,, Music.
8. ,, ,, ,, French.
9. ,, ,, ,, Chemistry.

A research on Psychological Tests of Mathematical Ability has been completed by Mr L. Fouracre, M.Sc., M.Ed.

THE UNIVERSITY, MANCHESTER.

Dr R. H. Thouless has continued the investigation of the physics of the psychogalvanic reflex phenomenon, particularly with relation to the connection between the polarisation change and the resistance stage.

Mr H. F. O. James has studied the problems of perceptive grouping, and of attention with reference to the psychogalvanic reflex.

Mr S. Wyatt has studied (in conjunction with Mr J. A. Fraser) repetitive processes in industry.

Miss Burnett has concluded a report on problems of repetitive work.

Professor Pear with the co-operation of Miss Isabel Burnett has continued an investigation into the acquisition of muscular skill. Miss Burnett and Dr Eliza R. Collie are engaged in acquiring skill in various muscular performances, making notes of the subjective experiences and of the objective progress made. It is hoped in this way to obtain some information about the actual processes of learning, and to proceed eventually to study the different methods of teaching with reference to the acquisition of skill.

Professor Pear in co-operation with Miss Edna M. Yates has begun an investigation into musical appreciation by persons untrained in music.

UNIVERSITY OF LONDON. KING'S COLLEGE.

1. Character Qualities in Boys (Research still in progress; preliminary account already published in *B.J.P.*).
2. Subjective feelings and Conational Response.

3. Influence of Conation upon Mental and Manual Performance.
4. Conative Control.
5. Conation in Judgments (Preliminary account communicated to B.P.S.).
6. Emotional Abnormalities.
7. A research upon *The Will-Profile* is to be begun shortly.

GEORGE COMBE PSYCHOLOGICAL LABORATORY, UNIVERSITY OF EDINBURGH.

(1) The correlation of subjects in the Intermediate curriculum.
(2) An investigation of the causes of backwardness in school.
(3) An investigation into character and temperament tests (continued).

THE UNIVERSITY, SHEFFIELD.

(1) The Prognostic Value of Intelligence Tests at the University stage.
(2) The Intelligence of brothers and sisters as measured by Tests.
(3) A Study in Children's Vocabularies.
(4) The Development of Reasoning in children.

In addition a student is doing post-graduate research work for the degree of Ph.D. on "Vocational Guidance with special reference to the Industries and Occupations of Sheffield"; while another student is doing research work for the degree of M.A. on "Problems of Secondary School Organisation in the light of Group Psychology."

UNIVERSITY OF BRISTOL.

Work on Care Committees.
Ability and Fatiguability.
Correlation of Abilities in College Students.
Relation between Intelligence and Character.
Reliability of certain short Methods of Examining.
Acquisitions of the Adult Mind: (*a*) knowledge, (*b*) technique.
Development of emotion in young children.
Enquiry into the Genesis of Adult and Pre-adult education based on anthropological and archaeological material.

BEDFORD COLLEGE FOR WOMEN (UNIVERSITY OF LONDON).

The Research on Practical Ability has been published, "A Study of Practical Ability," M. McFarlane. (Monograph Supplement, *B.J.P.*)

The study of special abilities required by Students in the Faculty of Arts and in the Faculty of Science carried on with the Students here will be published in the Spring—*Ability*, V. Hazlitt (Methuen & Co.).

THE UNIVERSITY, GLASGOW.

1. The mental ratios of sick children.
2. The rhythm of speech.
3. The perception of form.
4. Literary appreciation in children.
5. Subjective correlates of variations in output.

LONDON DAY TRAINING COLLEGE.

Students registered for Higher Degrees (1925–6).

"A Liberal Education in the Elementary School." (C. H. Green.)
"Attainment Tests in Secondary Schools." (J. A. Strugnall.)
"Temperament." (C. Bloor.)
"Curriculum of Girls' Secondary Schools." (N. Jones.)
"Causes of Disability in Arithmetic." (E. C. Wheeler.)
"Causes of Disability in Reading." (E. G. Hume.)
"Temperament." (E. J. Weeks.)

THE UNIVERSITY OF CAMBRIDGE. PSYCHOLOGICAL DEPARTMENT.

1. Researches into the psychological nature of mental and 'moral' defect. (Miss L. G. Fildes.)
2. An experiment in Vocational Guidance in Cambridge and the district. (Mrs L. C. Ramsey.)
3. Physiological and psychological disturbances of vision of visual performances produced by flickering light. (Dr R. D. Buchanan.)
4. Legibility of Type. (R. L. Pyke.)
5. Test Types for visual acuity and their arrangement. (Dr H. Banister.)
6. Experiments on certain temperamental characteristics. (Miss M. Mason.)
7. Visual perception of movement in the peripheral field. (M. H. Salaman.)
8. Experiments on 'Serial reproduction.' (J. Prasad.)

PUBLICATIONS RECENTLY RECEIVED

Christian Missions and Oriental Civilisations: A Study in Culture-Contact. By Dr Maurice T. Price. Shanghai: China, 1924. (Obtainable from Orientalia, Inc. New York, U.S.A.) pp. xxvi + 578.

To anybody who is interested in the development of a sound social psychology, based upon facts and not mainly upon speculations, this book may be most strongly recommended. It is a contribution of first rate significance to the extremely important questions arising from the contact of social groups. There is only one way by which these questions can be answered satisfactorily and that is by the intensive study of special groups in their relation to particular problems. Obviously the effects of the impact of Christian missions upon Oriental civilisations form an extraordinarily interesting and important field of study. It is here surveyed, with abundant reference to and quotation from first hand authorities, in a thoroughly impartial way. The study is well ordered. It deals with the whole subject under the following broad headings: How non-Christians react to Missions; The Influence of the Native Group; Analysis of Reactions: (*a*) of first impulsive reactions, (*b*) of more permanent responses, (*c*) rejection, (*d*) favourable reactions; Deciding for Group Candidacy; The Propagandic Impact as a whole. There are brief, and less important, appendices, dealing with Psycho-Sociological Analysis; and Motivation and Causal Sequence. There is also a good bibliography. It is a most valuable book.

The Origin of Man (pp. ix + 100) and *Man and his Superstitions* (pp. xvi + 278). By Prof. Carveth Read. Cambridge University Press. 1925. 5*s*. net and 12*s*. 6*d*. net.

These two books form together a second edition of Prof. Read's *The Origin of Man and of his Superstitions*, of which a critical notice was published in this *Journal* (1921, vol. xi, p. 340). The first volume, suggesting that Man passed through a stage in which he hunted in packs, has been enlarged and certain criticisms have been met. The difficulties of the first change of habit are lessened by considering the clumsiness of contemporary mammalia. A passage has been added in defence of the speculative method.

The second volume is a corrected impression of the last eight chapters of the first edition.

An Introduction to Social Psychology. By Prof. William McDougall. London: Methuen & Co. 1926. Twentieth edition: revised and enlarged. pp. xxvi + 440. 10*s*. 6*d*. net.

It is an extremely gratifying thing to anybody who is interested in the progress of psychology to find that the widespread interest in Professor McDougall's now famous *Introduction to Social Psychology* continues. For although it is undoubtedly attractively written and easy to read, it is a book which abounds with striking and original ideas, and probably more than any other book on psychology published in English since its first appearance, it has stimulated a great amount of important research. To this twentieth edition Professor McDougall has added supplements dealing particularly with his views concerning the "instinct of laughter" and summarising some of the recent psychological work on instinct in general. There is also a brief and not very illuminating or adequate reference to the *Gestalt* psychology. These additions are undoubtedly interesting, but the main value of the book is still to be found in the parts which stand as they were printed in 1908.

Social Psychology. A Text-book for Students of Economics. By ROBERT H. THOULESS. London: University Tutorial Press. 1925. pp. vi + 376. 5*s.* 6*d.* net.

This small book (which is intended to cover the syllabus in Psychology in the London B.Sc. degree in Economics) deals briefly with almost the whole of normal adult psychology, with emphasis on the questions of interest to sociology.

The discussion of instinct (which follows an introductory chapter on the scope of psychology) is based principally on McDougall's treatment, though Dr Thouless does not accept McDougall's whole scheme. Stress is laid on the modification of instinctive behaviour by the intervention of the higher thought processes, and there is an interesting chapter on 'sublimation' and other ways in which instincts become transformed. The account of cognition suffers, as is inevitable in a work of this kind, from extreme condensation—some important subjects, such as memory, being scarcely mentioned—but here, as elsewhere, Dr Thouless is to be congratulated on the excellent use which he makes of a small space. A novel feature of the work is the introduction of two chapters on the psychology of economic value, which should be of use in helping the student to avoid certain fallacies in economic theory. There are also chapters on will, morality, mental tests, aesthetics, and scientific and religious development. We might perhaps have expected that in a work intended primarily for economics students a chapter would have been added on the applications of psychology to industrial problems.

Throughout the book there are frequent references to experimental work, and the treatment of controversial questions is fair and well balanced. The book is attractively written, and should give the student a clear idea of the present position of those branches of psychology with which it deals.

Art and Counterfeit. By MARGARET H. BULLEY. London: Methuen & Co. 1925. pp. xiv + 101. With 188 illustrations. Price 15*s.* net.

This book is not only very beautifully produced, but it is a most interesting and valuable contribution to the psychological study of aesthetics. The purpose of the volume is not, indeed, to enter upon a psychological study, but to help any who may be concerned to distinguish between true and counterfeit works of art. It is the author's excellent plan to let artists and art critics speak for themselves. Thus each chapter consists of a series of extracts from the works of notable persons in which they put forward their very varied views on the subject dealt with. Notes, some of them of considerable importance, are added, and, what is more important still, every point is illustrated so as to show what is claimed to be an instance of a genuine and one of a spurious work of art. The topics specifically dealt with are the relation of art to imitation, the analysis of design, taste and the appreciation of art; and the way in which art may be regarded as connected with religion, morality and the spiritual life. In an appendix an account is given of a very interesting experiment which the author carried out with a large number of school children. They were asked to arrange in an order of merit reproductions of certain 'true' and 'counterfeit' works of art. Results are given for English children, some French children and a smaller number of Japanese girls. To readers of this *Journal* it may be interesting to note that among the 'controls' used were 55 members of the B.P.S. But these were not, according to the 'best' canons, wonderfully successful!

Altogether this book should prove a great deal more valuable to the psychologist than most of the elaborate 'original' treatises on psychological aesthetics which are produced.

Old and New Viewpoints in Psychology. By Prof. KNIGHT DUNLOP. London: Henry Kimpton. 1925. pp. 166. Price 10*s.* 6*d.* net.

This book consists of three public lectures delivered at the Johns Hopkins University, and two papers read before the American Southern Society for Philosophy and Psychology. The subjects dealt with are: Mental Measurements; Present Day

Schools of Psychology; Psychological Factors in Spiritualism; The Psychology of the Comic, and The Reading of Character from External Signs. The discussion of each of these topics is lively, extremely readable, somewhat informal and never very thorough. It is probable that the lectures served their purpose extraordinarily well. They must have been interesting to hear, and have raised many points for discussion. In the more 'permanent' form of a book they seem rather thin. They contain, however, a number of acute criticisms of recent movements in psychology.

NOTES ON RECENT PERIODICALS

Beihefte zur Zeitschrift für Angewandte Psychologie. 14 *a and b.*

Psychische Geschlechtsunterschiede. Ergebnisse der differentiellen Psychologie statistisch bearbeitet von Otto Lipman.

The chief interest for co-education of the question of differences in the two sexes does not lie in the mere existence but in the amount of those differences—whether or not they pass the limit which makes co-education possible or advisable; and further that the qualities and characteristics investigated may be of less importance as regards co-education than those it was impossible to investigate. The author asks, what is the difference in percentages of men and women among those persons whose performances rank (1) as normal between two chosen values, (2) as subnormal below the lower of the two, (3) as supernormal above the higher of the two? The middle half of the total number of persons dealt with determine (1) above, the remaining two quarters (2) and (3) respectively. Results are arranged according to (1) sources, (2) higher percentages of men over women in supernormal, lower in subnormal classes, (3) those qualities in which either men or women show a consistent superiority.

Relative superiority of performance in any subject of instruction is found always to accompany relative superiority of interest in that subject. Girls' drawings more frequently than boys' represent what the child knows rather than what it sees. The boy plays 'world' to the girl's 'home.' Boys' interests lie more strongly in activity, girls' in possession. Alteration of degree of difference between the two sexes at different ages can be noted, but reversal seldom. From 3½ to 12 boys tend to do relatively better, girls from 12–19; these differences relating to age should be investigated independently. In any school-year boys do relatively their best work at the end, girls at the beginning of a term. In spring, particularly during puberty, girls do relatively badly. The superiority of males is more often shown in bigger representation in the supernormal division, of girls in smaller representation in the sub-normal. Males show greater intervariability.

Part II gives the material used, the tables made thereform, and its statistical handling, with a bibliography.

Bulletin de l'Institut General Psychologie. 1923. *Nos.* 4–6.

This volume contains an account by *M. René Grousset* given at the College de France, of *Les Empereurs Chinois. Mécènes et Dilettantes.* He explains that the art development of any country or nation is but a reflection of the social and political milieu of the time and proceeds to demonstrate his thesis by an examination of the art of China in relation to the character and personal behaviour of the various imperial dynasties which succeeded one another in power.

Comment les peintres français ont vu la guèrre. (M. René Jean.)

Here we find a conference which called attention to the fact that French artists in their records of the Great War have broken away from the tradition of the Old Masters. They have made no attempt to glorify war nor to idealise it, but have rather emphasised its horrors with all the simplicity of direct vision.

Le Mysticisme de la Musique. (Ricciotto Candudo.)

This author declares himself sensible of the new 'moral values,' which have arisen since the war and feels the need of "plunging into a bath of many-souled humanity." All former expressions of religion, he thinks, have lost their appeal and we need a new manifestation of mysticism, which is to be found in music. This he believes to be the only expression of art capable of evolution, since it shows in its development an increase of complexity through multiplication.

L'Idéalisme dans l'œuvre de Puvis de Chavannes. (M. Léonce Bénédite.)

A life history and appreciation of the artist Puvis de Chavannes. With his personal touch he rejuvenated all subjects he depicted, understanding that the first importance was to be human and to speak the human language. He shared the opinion of J. F. Millet that in order to affect others, one must first be touched oneself. By this simple faith he renewed the art of his time.

Le Problème de l'homme insoulevable. (M. Louis Faire.)

An elaborate investigation of the problem involved in the difficulty of lifting a man. The boxer Coulon was the subject to be lifted and records were taken with camera and cinema of the results. The test was carried out under laboratory conditions and apart from those arranged to make the conditions as constant as possible, repeated tests were applied to both lifter and person to be lifted (during the seance and otherwise), of (1) muscular sensibility, (2) muscular reactions, (3) suggestibility.

Zeitschrift für Psychologie. Bd. 97. *Heft* 3 *and* 4.

Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrnehmung. Pt. 2. (Walter Ehrenstein.)

The experiments with moving figures described in Pt. 1 are now carried further and applied to other non-angular figures. The following are among the more important findings: (1) some colour alterations, part contrast, part mixture phenomena; (2) alterations of contour as a result of (*a*) contrast (edge against background), (*b*) apparent intrafigural movements; (3) the slower shifting in the field of vision of the more impressive part of a complex figure.

Die Wahrnehmungszeit. 2nd Article. (F. Haxelhoff and H. Wiersma.)

The experimenters, again using the localisation method, arrive at the following results: (1) Beginning with the threshold value, the perception time with increasing intensity of stimulus decreases quickly at first until the intensity reaches about 10 × threshold value. Then the decrease takes place more slowly; over 200 × threshold value it is scarcely noticeable and it looks as if for still stronger stimuli, stronger than could be attained, the perception time would remain unaltered; (2) Under the influence of co-existent inhibiting stimuli the perception time for adequate stimuli becomes longer. The difference is greater where the latter are weaker or the former stronger.

Über musikalische Begabungen zu sonstigen Anlagen. (R. Miller.)

Stimulated by and doubtful of the results given by Haecker and Ziehen (*Zeitschrift für Psych.* IX, 3–6), on the subject of musical talent, the author carried out a careful enquiry among the records of Friesing training college (male students). In contradiction to the above, he found a clear correlation between musical and mathematical talent, higher than that between the former and drawing talent. Good general gifts in most cases include good musical gifts, and good musicians are in nearly all cases highly gifted in general.

Experimente über die Grossenkonstanz. (G. Katona.)

As a contribution to the analysis of visual perception, its nature and origin, the author makes an experimental enquiry into the difference between perceived size as affected by distance and the actual retinal image. The experiments showed that the perceived image is not merely the totality of stimuli influenced by effective "reproductive" elements, and disproved the validity of the "transformation" hypothesis of the effect of the sensation of distance through contrast. The enquiry now proceeds from the fact of the unity of perception—the structional connection of its elements. This structure is given primarily by the mutual relation and inter-effectivity of all the stimuli elements; thus, even apart from the second structural influence of subjective factors (active attention, etc.) it does not correspond to the mere sum of stimuli, but has what may be called a primary objective structure. In the case under immediate discussion, the difference between perceived size and retinal image size is only there when the primary structure of the perception is determined by either distance or dimness. These are the factors: how they work is not and perhaps cannot be determined.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

February 13, 1926.	Judgments of Intelligence Based upon Interviews, by Dr E. H. Magson.
	Indications of a Logarithmic Law in Learning, by R. S. Bartlett.
March 13, 1926.	The upper limits of the growth of Intelligence, by Miss Eliza A. Collie.
	Some further notes upon Work Curves, by S. J. F. Philpott.

SECTIONAL MEETINGS.

Education Section.

January 4, 1926.	The Nature and Training of the Imagination, by Miss Margaret Drummond.
March 8, 1926.	(With the Medical Section.)
	The Definition and Diagnosis of Moral Imbecility, by Dr Cyril Burt, Dr M. Hamblin Smith, Dr W. Rees Thomas, Dr F. C. Shrubsall and Dr A. F. Tredgold.

Industrial Section.

February 20, 1926.	What is meant by Skill in Industry? by Prof. T. H. Pear.

Medical Section.

January 27, 1926.	Investigations into the Predisposition to Breakdown, by Dr Millais Culpin.
February 24, 1926.	A Hypothetical Mental Constitution of Compulsive Thinkers, by Dr J. T. MacCurdy.
March 8, 1926.	(With the Education Section.)
	The Definition of Diagnosis of Moral Imbecility, by Dr Cyril Burt, Dr M. Hamblin Smith, Dr W. Rees Thomas, Dr F. C. Shrubsall and Dr A. F. Tredgold.

CAMBRIDGE: PRINTED BY
W. LEWIS, M.A.,
AT THE UNIVERSITY PRESS